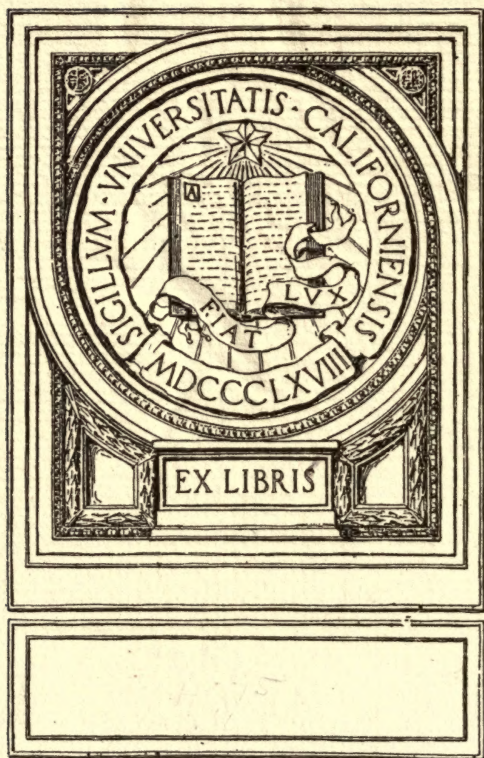


THE LIFE OF  
ROBERT HARE  
EDGAR FAHS SMITH

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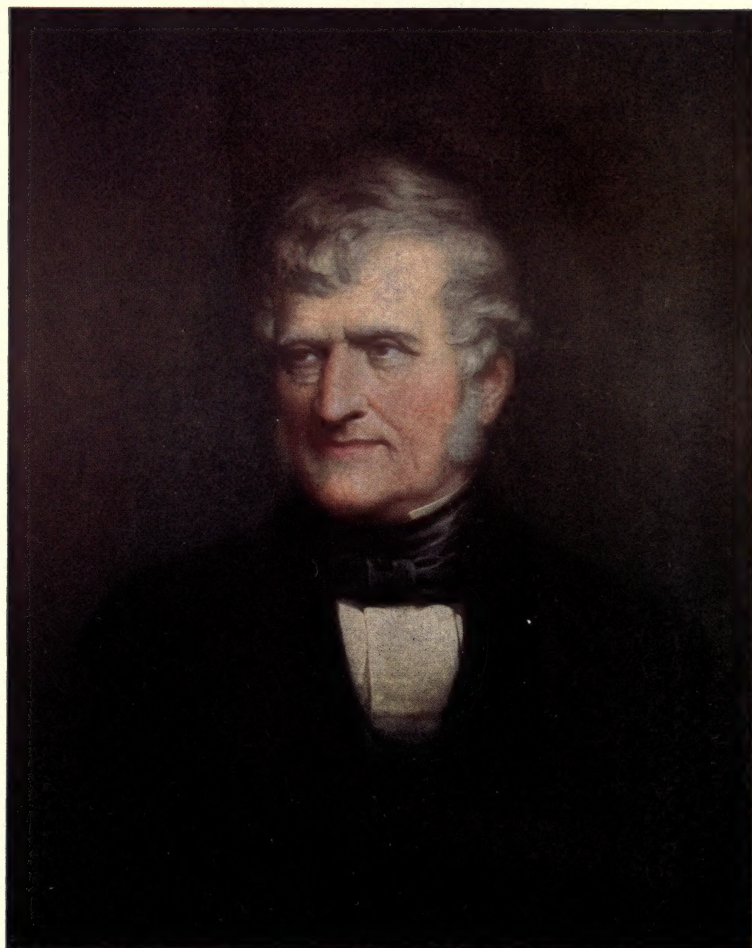
THE LIFE  
OF  
ROBERT HARE  
AN AMERICAN CHEMIST







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ROBERT HARE

From the Oil Portrait in the University of Pennsylvania  
Commenced by Neagle, 1858  
Finished by J. L. Williams, 1877



THE LIFE  
OF  
ROBERT HARE

AN AMERICAN CHEMIST  
(1781-1858)

BY  
EDGAR FAHS SMITH  
PROVOST OF THE UNIVERSITY OF PENNSYLVANIA

*WITH A PORTRAIT IN COLOR  
AND FOUR DOUBLETONES*



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**TO  
MY MOTHER**

**360388**





## PREFACE

THIS volume contains the life story of one of the greatest scientists of our country. His chief delight was in chemical pursuits, although his attachment to physics was also great. He was a true pioneer in these divisions of science. His experimental contributions were of a very high order in their day. They commanded respect and admiration then and continue to do so in the present because they represent the beginnings of much that has come to be of prime importance.

When, in the future, the contributions of America's earliest representatives in the many fields of science are scanned more closely, an abundance of noteworthy material will be discovered and our country, though young, will be found to have given her share to the sum total of human knowledge.

The purpose of the writer has been to assemble the labors of Robert Hare in such a form that students of chemistry may learn to know him better, and realize the exalted place to which he is entitled in the history of chemistry in this country. He was a chemical philosopher with keen and origi-native powers. It is remarkable that he should have achieved so much when his preparation was so meagre. He blazed the way by his experimental work and in his theoretical observations in chemical constitution. His "Compendium of Chemistry," now antiquated, was a store-house of original observations. He had no model. He advanced independently and, as his knowledge increased, developed new lines.

The writer, at one time ignorant of Hare and of his unique as well as remarkable labors, has become, through an intimate study of his work, an enthusiast in regard to him and, therefore, has ventured to present this story, told largely

by Hare himself in a series of unpublished letters, and in other documents which were practically buried in forgotten journals and pamphlets, while some did appear in the *American Journal of Science*, to which Hare contributed the greater portion of his experimental conclusions. The writer's sincere thanks are due the editorial staff of the *Journal* for permission to use this precious material so generously, and to the American Philosophical Society for the extracts made from its Proceedings and Transactions, as well as to Fisher's "Life of Benjamin Silliman," from which were gleaned data of a more intimate character regarding the subject of this sketch and his devoted friend Silliman. To T. Truxton Hare, Esq., a great-grandson of Robert Hare, as well as to Dean John Frazer, of the University of Pennsylvania, the writer would express his great indebtedness for many unprinted letters and the privilege of using portions of others which illuminated many points in this life story, which otherwise would have lacked completeness.

Robert Hare, an American chemist, will surely live in the memory of all who become acquainted with him through his epoch-making contributions to that science which is so closely interwoven with the welfare, comfort and happiness of mankind.

E. F. S.

UNIVERSITY OF PENNSYLVANIA  
PHILADELPHIA



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# THE LIFE OF ROBERT HARE

## AN AMERICAN CHEMIST

### FIRST PERIOD

1781-1818

IN Philadelphia, during the last two decades of the 18th Century, occurred many of the most important events in the history of the Western World. That city was then the leading city of our country. It was the richest city. It led in all important undertakings of the day. Congress had long deliberated there. In Philadelphia, the Declaration of Independence was written, and the Constitution of the United States was framed. Noted for its wealth and for its influence in all matters of moment to the young and growing Nation, Philadelphia was accorded the unenviable distinction of being very wicked. Several devastating epidemics of yellow fever had occasioned the thought in the minds of some people that God was punishing the city. Be that as it may, the people were sobered by these visitations of the plague, and many instances of self-sacrifice were recorded. It was during these dreaded times that the celebrated Dr. Benjamin Rush rendered such signal service, claiming that his administration of mercury and blood-letting was the only hope of recovery from the fatal disease, thereby contributing to the science of medicine.

In Philadelphia, in 1781, Robert Morris, the lofty-minded and far-seeing financier of the Revolution, founded the first and most opulent bank—the Bank of North America.

In Philadelphia, as nowhere else in America, flourished science. Franklin was omnipresent; his wonderful experiments on electricity had received world-wide recognition. Joseph Priestley, seeking a shelter from persecution, had

come from England. He associated freely with the men of science in the city, meeting them in their homes and at intervals in the hall of the now venerable American Philosophical Society, upon the pages of whose Transactions later appeared his final efforts to establish the strange doctrine of phlogiston. In Philadelphia, in 1792, was founded the oldest chemical society in the world—the Chemical Society of Philadelphia. It is to be regretted that so few of its publications have come down to posterity for, though its existence was short and its endeavors were of a pioneer character, they were of no mean order, contributions of far-reaching importance being made by it to the scientific world.

In Philadelphia, during this important period, began the life of one of America's greatest chemists—Robert Hare—born on the seventeenth day of January, in the year of our Lord, one thousand seven hundred and eighty-one. One publication describes him as “the greatest American light of chemical science,” while another ranks him with Sir Humphry Davy, Volta, Priestley and Berzelius. He made his advent into the field of chemical discovery when about twenty years of age, and for fifty years he was regarded as an unimpeachable authority in all matters pertaining to chemical research.

Upon tracing his lineage, we find that his father was Robert Hare, an Englishman by birth, who came to America in 1773. He was an educated man, of good family and of refined tastes, who married Margaret Willing, daughter of Charles Willing, whose family ranked high in the social world of Old Philadelphia. The elder Hare was not without honor in the country of his adoption, as evidenced by his membership in the Convention which framed the first Constitution of Pennsylvania; by his becoming Speaker of the Senate of the State; and by his occupancy (1789–1805) of a seat in the Board of Trustees of the University of Pennsylvania. To Robert Hare and Margaret Willing were born



five sons and one daughter. The younger Robert attained to the greatest distinction, although his brother, Charles Willing Hare, reached a very honorable position at the bar. John Powel Hare, the sixth child, later known as John Hare Powel, after adoption by his maternal aunt, Mrs. Elizabeth Willing Powel, became Secretary of the U. S. Legation at London; subsequently, he served as Major of Volunteers under General Thomas Cadwalader, and in due time was advanced to a Colonelcy in the Regular Army of the United States on the Staff of General Winfield Scott. Impatient of inactivity, he left the Army. He was a charter Trustee of Lafayette College and at one time, like his father, a member of the State Senate. There fail any records of the life histories of the remaining children of Robert Hare and his wife, Margaret.

Robert Hare, the elder, was a celebrated brewer. "Hare's American Porter" was widely known, although beer was brewed in Philadelphia for several years before the Revolutionary War. Westcott<sup>1</sup> remarks, "Hare's brewery stood at the S. E. Corner of Callowhill and New Market Streets. On the evening of the first of April, 1776, the brewery was entirely destroyed by fire, making a conflagration which was long talked of in the city." And in the *General Advertiser* for November 2, 1790, there appeared this notice:

"Sunday morning, about 4 o'clock, the brew house of Mr. Hare, in the Northern Liberties of this City [Philadelphia] was discovered to be on fire, and notwithstanding the utmost exertions of the citizens, a great part of it was burnt."

Early prints report, however, that the plant was rebuilt and the business resumed with its usual industry.

Returning to Robert Hare, the younger, a very natural desire manifests itself to know as much as possible of his

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<sup>1</sup> "Biographies of Philadelphians," by Thompson Westcott, vol. ii, Part I.

early youth. Singularly enough the very meagre accounts of him fail to mention this interesting period, and one wonders in what way were acquired the foundations for the astounding mental equipment displayed by him in his young manhood and more mature years. Inference would ascribe to his father full and sole credit for having personally supervised his youthful tutelage, and this inference seems conclusive from the certain knowledge that in the instance of his other sons, Hare, the elder, "imparted the rudiments of a good classical education, and besides planted in their hearts the stern sense of individual responsibility, love of truth, and high principles which marked their whole intercourse with the world." Following the years of early preparation, young Robert assisted his father in business; it is said of him, however, that cherishing a love for the physical sciences—particularly chemistry—he entered "the Chemical School of the University of Pennsylvania." There was then in existence no distinctly independent School of Chemistry in the University, and probably what Hare did was to choose the lectures on chemistry, which, at the close of the 18th Century and the beginning of the 19th Century, were delivered by Woodhouse in the "Anatomical Museum." This museum was a frame building on South Fifth Street, directly opposite the State House grounds. These lectures of Woodhouse also attracted other students, whose purpose it was to follow chemistry rather than medicine. Woodhouse ranked very high among his associates, and as he strongly emphasized laboratory work, Hare would have been drawn to him. It is also quite possible that the laboratory manual of Woodhouse, and his editions of more dignified foreign works, constituted the literature upon which Hare thrived, for he was a genuine enthusiast and tireless in his search for knowledge.

Mention has been made of the Chemical Society of Philadelphia. In it were assembled those who desired to prosecute



chemistry—in short, all persons who meant to become specialists. The knowledge<sup>2</sup> possessed of the aims, purposes and accomplishments of this organization but increases one's desire for further information concerning it.

Evidently it supplied a want felt by Hare, for he was a junior member of the Society, and at various times appeared as a Committeeman or subordinate officer.

While prosecuting his studies with Woodhouse, Robert Hare engaged in very independent research, which culminated in the presentation, in an annual address to the Chemical Society of Philadelphia, in 1801, of an exhaustive and illustrated account of the oxyhydrogen blowpipe, a discovery of the highest importance. This discovery gave the indubitable evidence of a highly philosophical mind in its author, for then the notions of the real nature of combustion were extremely vague and that Hare should have had the acumen to conceive that a stream of oxygen and hydrogen burning together would produce so intense a heat was extraordinary.

It was a splendid triumph for him. And when it is realized that from this discovery sprang, among others, the lime light or Drummond light, universal gratitude is due this youthful adventurer into the field of pure and applied science. The intense heat of the oxyhydrogen blowpipe enabled Hare to fuse platinum, so that some years after the discovery had attained a higher degree of perfection, a student of Hare, familiar with the compound blowpipe, set forth to found in this country an industry in the working of platinum. Indeed, Bishop's Platinum Works of to-day is the modern development of Joachim Bishop's pioneer effort. From the beginning its several steps were crowned with remarkable success.

The exact title of Hare's communication, presented in 1801 to the Chemical Society of Philadelphia, was a "Memoir of the Supply and Application of the Blow-Pipe.

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<sup>2</sup> "Chemistry in America," Appleton & Co.



Containing an Account of the new method of supplying the Blow-pipe either with common air or oxygen gas; and also of the effects of the intense heat produced by the combustion of the hydrogen and oxygen gases."

Chemists of Europe have adopted the plan of reprinting memorable contributions of science in a series such as the "*Klassiker der Exakten Wissenschaften*." Should similar recognition be made in America of the achievements of her scientists, then surely the above Memoir ought to find place therein, for, without question, it is a genuine classic. Now and then copies of this publication appeared in second-hand book-stores, although recently there has been printed an exact copy of this text with complete illustrations.<sup>3</sup> It is, therefore, quite unnecessary to reproduce the same here. Let it suffice to note that with the flame from the oxygen and hydrogen gases Hare succeeded in fusing heavy spar, alumina, silica, lime and magnesia, while platinum, gold and silver "were thrown into a state of ebullition," and many other remarkable behaviors of metals observed.

In the winter of 1802, Benjamin Silliman arrived in Philadelphia. He had been recently chosen to the Chair of Chemistry and Natural Philosophy in Yale College. It was his desire, before entering upon his duties, to hear the lectures of James Woodhouse in chemistry, Benjamin Smith Barton in natural history and Caspar Wistar in anatomy. Friends had recommended him to take lodgings in a wedge-shaped house at the S. W. corner of Dock and Walnut Streets. It seems that this was a favorite place with a very select class of gentlemen; and in that company he met Robert Hare. Why the latter should live in a boarding house when his father's home was wide open to him cannot be determined. To chemists, however, the meeting of Hare and Silliman, with all that followed, is of vastly more interest. In the annals

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<sup>3</sup> "*Chemistry in America*," Appleton & Co.

of chemistry there have been recorded instances of extraordinary friendships formed by persons strongly attracted to one another by common interests. Thus, in the friendship of Wöhler and Liebig there is an example of two gigantic minds working in harmony in their chosen field. This also must have been true of Gerhardt and Laurent; so that in the intellectual union of Hare and Silliman there is an additional happy combination of the better elements in human nature. These three instances, with doubtless many others, should inspire the formation of still others, for, thereby, good alone will ensue.

Silliman was exceedingly happy in the company of his fellow-boarders, notwithstanding he must at times have been surprised at their conduct, for he wrote, "I do not remember any water drinkers at our table or in the house. . . . Porter and other strong beer were used at table as a beverage. As Robert Hare was a brewer of porter, his was in high request, and indeed, it was of an excellent quality. . . . There were no outward manifestations of religion in our boarding house. Grace was never said at table, nor did I ever hear a prayer in the house"; but, he continues, "rarely have I met with a circle of gentlemen who surpassed them in courteous manners, in brilliant intelligence, sparkling sallies of wit and pleasantry, and cordial greeting both among themselves and with friends and strangers who were occasionally introduced." And in this brilliant group Robert Hare was a bright, particular star.

Silliman did not wholly appreciate Woodhouse, for it is recorded in his diary:

"The deficiencies of Woodhouse's courses were, in a considerable degree, made up in a manner which I could not have anticipated. Robert Hare, my fellow-boarder and companion at Mrs. Smith's, was a genial, kind-hearted person, one year younger than myself, and already a proficient in chemistry upon the scale of that period." Then follows an account of how Hare, on hearing the reason for Silliman's



presence in Philadelphia, kindly extended his friendship and assistance. Together they persuaded Mrs. Smith, their high-spirited, efficient and indulgent hostess, to let them arrange a small laboratory in a spare cellar-kitchen, in which they worked in their leisure hours. The latter were probably those which Hare could spare from his business and Silliman from his lecture hours and private study. Hare's thoughts constantly gravitated toward improvements in the apparatus for the burning of oxygen and hydrogen. It was his hope that the heat might be made more intense. This he expected to effect by getting really pure oxygen from oxymuriate of potassa (potassium chlorate). As chemists then were not aware that the addition of a little black oxide of manganese to the chlorate considerably accelerated the liberation of the oxygen gas, thereby permitting the use of glass flasks or retorts, he considered it necessary to expose the chlorate in stone retorts to furnace-heat and Silliman's diary adds:

"The retorts were purchased by me at a dollar each, and, as they were usually broken in the experiment, the research was rather costly; but my friend furnished experience, and, as I was daily acquiring it, I was rewarded, both for labor and expense, by the brilliant results of our experiments."

The friends frequently commented on the danger which surrounded the method of storing the gases. The possibility of mixing was constantly before them. Explosions did occur. It would seem that Silliman in particular was much concerned on this point. Later, at his home in New Haven, he "contrived a mode of separating these gases so effectually that they could not become mixed."

It was at this time—the winter of 1802–1803—that Hare exhibited to Dr. Priestley, Seybert and others his compound blowpipe and the intensity of the oxyhydrogen flame. Priestley "gave them the credit of being quite original." What were the thoughts of the noble old dissenter on this



occasion? What joy must have come to young Hare in presenting his discoveries to the noted chemist of England and to the real leaders of chemical thought in this western world! It was a rare privilege to disclose to the discoverer of oxygen a use for it which probably never entered the discoverer's thoughts. Hare's invention, if such it might be called, was the talk of at least those scientifically inclined among the residents of Philadelphia. It must have afforded the staid members of the American Philosophical Society particular pleasure to listen to the young scientist (1803) telling how in the presence of Woodhouse and Seybert he had "completely dissipated globules of platinum about one-tenth of an inch in diameter," and, with his friend Silliman, had fused strontianite from Argyleshire, Scotland; whereupon they then quietly proceeded to honor him with election to their distinguished company. This occurred on January 21, 1803, when Hare had just reached his twenty-second birthday, and is recorded in these words:

"Election of two new members:

Robert Hare, Jr., of Philadelphia;

Ben. Count of Rumford, of Great Britain."

To be chosen simultaneously with Count Rumford was additional evidence of the honor in which he was held. It must also be remembered that in 1839 the Academy of Arts and Sciences conferred on Robert Hare, in recognition of his great invention, The Rumford Medal, granted for the first time.

One month after his election to the venerable American Philosophical Society, he was added to the

"Committee on Minerals:

Woodhouse, Church, Jacobs, Barton, Cooke,

Hewson, Hare,

to examine future donations."

That he received this appointment with serious appreciation of his duties seems evident from the following letter to an early friend of congenial tastes, residing in Baltimore:

“ Philadelphia, 1804

“ Dr Sir

Many months have elapsed since I have had the pleasure of addressing you in the epistolary style. My silence, however, has not arisen from the want of disposition to communicate with you, for this is always agreeable to me, as you are among the few in whom I find a congeniality of feeling on subjects which to me are very interesting. The fear of proving myself a dull correspondent has prevented me from writing on indifferent topics and none others have offered themselves. Nor indeed can that upon which I now resume my pen be interesting to you in any other light than as relative to the extension of a favorite branch of science. Mr. Thos. P. Smith, a young gentleman of our city, of an ardent and inquisitive mind and not unenlightened by the rays of science or of genius, having in a tour through Europe made a considerable collection of minerals, had the misfortune to lose his life when he had just arrived in sight of that native country for the ornament and improvement of which his researches had highly qualified him.

But though death extinguished all hope of benefit from future exertion of his talents, and industry the fruits which had already resulted from them bequeathed to a publick association insured advantage from the past. The bequest of his minerals to the American Philosophical Society may certainly be considered as promising publick benefit. It may serve as a nucleus on which a respectable cabinet may be formed. Few are so well aware as you of the importance of such an establishment. To you it is well known how little the department of mineralogical science has been explored or understood by our countrymen. The fact America boasts not of a single school for this science!

The minerals of Mr. Smith have lately been received by the Society and the names of Drs. Woodhouse, Barton, Sey-



bert and Coxe have been enrolled as a committee for the arrangement of them. Of this committee I also am a member, although as a practical mineralogist I can lay little claim to this appointment, yet I am willing to earn a little thereto by the bestowal of time and pains and I am ready to acquire information from any source whence it may be obtained. I presume that there must be various methods of exhibiting minerals in the various cabinets of Europe. That proper for a Society must differ from that suiting an individual as in the former case they should be seen without personal attendance and therefore should be constantly exposed to view and yet be defended from peculation. Our Committee have resolved to procure pictures of cases and thus to have the better opportunity of meeting with one which may suit their purpose. As I believe you to possess both taste and experience I will take it as a favour if you will sketch out any plans that you may have seen or that may suggest themselves to you and send them to me. The room they are to occupy faces the north and east. We have as yet had no election nor shall we until October.

Be so good as to present my respects and good wishes to your father's family and believe me

With regard,

Robert Gilmore Esq<sup>r</sup>, Jun<sup>r</sup>  
Baltimore.

Sincerely yours,  
R. HARE, Jun<sup>r</sup>."

During the year Hare made two verbal communications to the Society on subjects related to his oxyhydrogen blow-pipe. These were regarded worthy of publication, and it is therefore not surprising to learn that at the annual election (1804) of the Society, Hare was chosen a curator and continued in this office for ten successive years. Sometime later he filled the honorable position of councillor, although his attendance upon meetings at this time was scarcely what one might expect from so enthusiastic a scientist. Yet this may



have been in part due to the attention he was obliged to bestow upon business.

In the winter of 1803-1804 Silliman returned to Philadelphia. He writes, "I attended, as before, the course of chemistry and anatomy and resumed my private labours with Robert Hare." On leaving Philadelphia finally, in 1804, Silliman began the installation of his laboratory at Yale. He worked in most humble surroundings, but apparently never for the briefest period lost sight of the great possibilities of the oxyhydrogen blowpipe, and succeeded in achieving many things which were but imperfectly touched upon while with Hare. There are evidences that these friends communicated at intervals, though little of the actual correspondence is now to be found. Hare working quietly in Philadelphia—now in the basement laboratory at Dock and Walnut Streets, now in the laboratory of the Chemical Society, or in the brewery of his father—improved upon his original device, and with each improvement obtained results which from time to time found a way to the public and impressed them with the marvellous skill and insight of the young chemist. Silliman, appreciating Hare's achievements, was probably responsible for the high honor accorded his friend by Yale College, when it conferred upon him the degree of Doctor of Medicine, in 1806. This was Hare's first academic recognition.

Reverting again to the blowpipe, be it remembered that every chemist is quite familiar with the ordinary blowpipe and its numerous uses, and, further, knows the difficulties attendant upon all efforts to preserve it in continuous action. All these—uses and disadvantages—were quite familiar to Hare. He knew that the attachment of a bellows would obviate some of the inconveniences, but he sought to do more. He aimed to have a steady gas current pass through the blowpipe, first a current of air, later one of oxygen and hydrogen under constant, steady pressure—actually burning these

gases at a common exit tube, thus obtaining the intensest heat yet attained. It is no wonder that chemists were deeply impressed with the results. In our own day the remarkable reactions induced by the electric arc astonished the chemical world, and the enthusiasm and thoughts thus developed are nothing more than a return of the feelings of our forefathers in the domain of chemical research.

It must not be forgotten that originally the oxy-hydrogen flame was not Hare's objective. No, his desire was to improve the ordinary blowpipe. Having accomplished this by his new hydrostatic arrangement, there followed the introduction of oxygen into the blowpipe with the observance of an increased intensity of the flame. The crowning effort was the extension of his hydrostatic device to accommodate in separate apartments the gases oxygen and hydrogen, expelling them under pressure, uniting them and igniting them at a common orifice, when there was observed the intense heat of the flame, which was quickly followed by its application to the performance of the unusual things to which reference has already been made. Hare's achievement, then, was in part a mechanical invention, and in part the discovery of an unexpected source of the intensest heat. It was unique in many respects and brought results of far-reaching import. The scientific world took early cognizance of the same. The author of these humane benefits continued industriously at work on improvements in the apparatus and the extension of the use of the flame. His friend Silliman and others followed his example, but Hare always led, "succeeding in later years in constructing the apparatus on a gigantic scale, with large vessels of wrought iron capable of sustaining the pressure of the Fairmount Water Works, and that with this wonderful combination he was able to fuse at one operation nearly two pounds of platinum."

This remarkable, epoch-making work of Robert Hare,



however, was not suffered to pass without counter claims as to like discoveries. Clarke, of Cambridge, England, wrote a book, about 1819, upon the "gas blowpipe." In it he ignored absolutely the discovery of Hare, the researches of Silliman and others, and actually appropriated all that they had brought to light. It was not in the nature of Hare to be silent under such provocation, so he made an elaborate defence of his claims and those of Silliman. This may be read in the *American Journal of Science* for the year 1820. The protest was full and spirited. Clarke received it but remained silent. Hare began his objections to Clarke's publication with these words:

*"Hoc ego versiculos feci, tulit alter honores, etc."*

His indignation must have been great. Silliman stoutly maintained throughout the controversy that Hare was "the real inventor of the compound or oxy-hydrogen blowpipe."

Still another claimant of this discovery was a certain Mr. Maugham of the Adelaide Gallery in London, who asserted that he had contrived a blowpipe by which he had fused twenty-five ounces of platinum and that Hare had purchased one of his make when on a visit to London. Hare conclusively proved the worthlessness of Maugham's claim, and spurred on by friends, who foresaw even greater results from the invention, he was prevailed upon to patent his discovery. This, however, did not occur until 1845.

Hare labored, as time permitted, in perfecting his discovery, and gained by means of his investigations a wide knowledge of chemical bodies. Queer suggestions were made, from time to time, as to the application of the oxyhydrogen flame. Thus, a Thomas Skidmore, Esq., of New York (1822) professed to have "discovered, within three or four months back, that if the flame produced by the combustion of hydrogen gas, issuing in combination with oxygen from



the compound blowpipe of Hare, be plunged below the surface of a vessel of water, it continued, notwithstanding its submersion, and actual contact with, this element, to burn, apparently with the same splendour as it does in the common atmosphere." After detailing a number of little experiments "which have lately amused me and my friends," he adds, "I am not much disposed to indulge in speculation on the applications, which, in the course of the progress of science, may be made of these facts; yet I cannot refrain from observing that the possibility of effecting the combustion of most substances, with an agent so energetic as the heat evolved by the gases in question, seems to point distinctly, among other things, to their employment as a *submarine instrument of naval warfare*. From the experiments I have made (*and these too, with means having no reference whatever*, to the accomplishments of such a purpose), I am fully satisfied that success may be commanded" . . .

"The employment of Hare's jet to illuminate light-houses and signal reflectors under the names of Drummond light and Calcium light is only another example of the mode of ignoring the name of the real discoverer, of which the history of science presents so many parallels."

The departure of Silliman from Philadelphia was felt by Hare. It could scarcely have been otherwise, for in the words of the former—"I was often surprised, as well as gratified, to find in Mr. Hare an extent of comprehension as well as minuteness of conception and information which made his society a constant scene of entertainment and instruction to me; and in fact, our conversations became so frequent and long on chemical subjects, that our companions in the house often rallied us on our devotion to this pursuit."

But, before Hare were ever his imperative business duties, and they must have sadly interrupted his experimental work. He must have yearned for time in which to devote himself

to the subjects nearest to his heart and which occupied the greater part of his thoughts. In addition to his business affairs and his scientific researches, he indulged in the writing of letters to the public through the medium of the daily press on burning topics of the day. In all this work Hare showed himself to be a true patriot, and a many-sided person with a broad vision. However, before these letters receive consideration, it may not be out of place to record the attempt of personal friends and men of science made to place him in a position where his best efforts could be given to his favorite studies. Woodhouse, Professor of Chemistry in the University of Pennsylvania, died in 1809. At once the Trustees of the Institution were recipients of letters advocating the appointment of Hare.

“ Dear Sir:

During the first years of the establishment of the Medical School of Philadelphia it was required that every student who had not graduated in some College should be obliged to attend the course of lectures upon Natural Philosophy, previously to his being admitted to an examination for a medical degree in our University. This rule was imperfectly complied with during the greatest part of Dr. Ewing’s Provostship, but has been neglected for several years to the great injury of our Medical School and of Medical Science in our country.

Permit me to suggest to you the necessity of appointing a Professor of Natural Philosophy *for the express* purpose of teaching that important branch of science to students of medicine in the extensive way in which it is taught in European universities. Such a course of lectures will not interfere with the instruction in Natural Philosophy given to candidates for degrees in the Arts by the Provost of the University. They will be addressed to persons of a more advanced age, and will embrace many objects especially necessary and useful to students of medicine. Should such a professorship be instituted,



there will be no difficulty in filling it. Mr. Robert Hare's extensive knowledge in Natural Philosophy, and all its collateral subjects, points him out as a most suitable person for that purpose. His splendid talents and ardor in scientific pursuits, I have no doubt, would add greatly to the reputation of our Medical School, and to the honor of our City, and State.

Should you think proper to propose the professorship I have mentioned, suppose you add to it at the same time—a professorship of rural economy?

Novem. 25

From Dr Sir

1809

Yours very respectfully

George Clymer, Esq.

BEN<sup>j</sup>. RUSH."

Mr. Clymer was a Trustee of the University and Dr. Rush had been a professor in the Medical School from its inception. Both of these gentlemen had affixed their names to that immortal document—the Declaration of Independence.

And Dr. John Syng Dorsey wrote on June 12, 1809:

"In the year 1798, I commenced the study of medicine in the University of Pennsylvania, and attended a course of Lectures on Chymistry delivered by Dr. Woodhouse, in whose laboratory I frequently met Mr. Robert Hare, jun., who was at that time engaged in the study of Chymistry. Mr. Hare was zealous in the pursuit; and in the Chymical Society of which we were both members, he always took an active and conspicuous part. Engaged in similar studies, I passed no inconsiderable portion of my time in company with Mr. Hare, and for several years was the frequent witness of his experimental researches, which have led to results, in my opinion, highly important, and I know of no chymical discovery which has been made in America, more brilliant than one of which Mr. Hare is the author.

I have therefore, from what I know of Mr. Robert Hare, jun., every reason to believe him perfectly qualified to teach the science of Chymistry. His mind I believe to be pecul-



ially adapted to this pursuit, and I have no doubt that he will discharge the duties of it with advantage to his pupils, with reputation to himself, and with honour to any institution with which he may be connected."

While in an epistle to E. Bronson, Esquire, June 15, 1809, Benjamin Silliman, of Yale College, said:

"I consider the gentleman who is the subject of this letter as one of the fairest hopes of the science of this country; especially should he, before the ardour of his mind has abated, be able to devote any considerable portion of his time and exertions to the cultivation of science."

Again: "From the knowledge we have of Mr. Robert Hare's Chymical abilities, we have no hesitation in declaring, that we believe him qualified to supply the vacancy occasioned by the death of the late Professor of Chymistry in the University of Pennsylvania.

ROBERT PATTERSON

Philadelphia June 15, 1809.

JOSEPH CLOUD."

And Dr. Chapman wrote to Joseph Hopkinson, Esq., author of "Hail Columbia," as follows:

"Dear Sir:

. . . I am detailing with unnecessary minuteness the merits of Mr. Hare. They have already been acknowledged on all hands. Those who know him best and are competent to decide, have borne evidence to the extent of his acquisitions in the philosophy of the science, to the dexterity of his manipulations, and the peculiar aptitude of his mind to the cultivation of those pursuits. The late Dr. Woodhouse, it is known to many, entertained the highest respect for his attainments, and often regretted, that a genius so well adapted to Chymistry could not be applied altogether to its improvement.

With respect to the incapacity of Mr. Hare, arising out of his want of a medical education, to which you allude, I must say that it strikes me with no force and that it can hardly be pressed, I presume, by his opponents. . . ."

While a testimonial from Dr. William P. Dewees reads:

“ Having perused a letter from Thomas Fitzsimmons, Esq., in which it is stated that it would be agreeable to the Trustees of the University, to receive information in regard to the chymical abilities and acquirements of the Candidates for the Professorship of Chymistry, I should deem myself wanting in justice, were I, when called upon by any of the Candidates to suppress the information which circumstances may have afforded me. On this ground I do not hesitate to communicate that knowledge of Mr. Robert Hare, jun., which I have derived from an intercourse of several years. In 1799, I first became acquainted with this gentleman, and in the following year found him engaged in the pursuit of Chymistry, both by study and experiment. Since which time I have been a frequent visitor at his laboratory, and have been witness of his researches, of which he has always given the most satisfactory explanation. I have frequently proposed to him questions that to me seemed obscure, and have always obtained sufficient elucidation. Chymistry has been, in fact, the most frequent topick of our conversations.

On grounds such as these I have not hesitated to adopt the opinion, that this gentleman's mind is peculiarly fitted for the investigation of Chymical science; and I consider him well acquainted with it in its various relations, to the arts and to medicine. Its connexion with the latter has been the most frequent subject of our disquisitions.”

There was opposition to Hare, based entirely, however, on the fact that he had not been educated in medicine. The University was not prepared to take the responsibility of introducing a non-medical man as teacher into its faculty, so the choice of successor to Woodhouse fell upon John Redman Coxe, who had proposed a plan for electric telegraphy, “ which long ante-dates any other American suggestion on this subject since the days of Franklin.”



It is very plainly indicated in the preceding letters that Hare's friends and admirers thought he should, by all means, be relieved from the strain of business cares; his talents for experimental science were so marked that every opportunity should be afforded him to devote himself to those things which appealed most strongly to him. His entrance, however, into a free untrammelled scientific career was not yet—but its day was approaching. Filial duty no doubt figured largely in his life work. And, from the subjoined bill he had become in truth a partner with his father, hence burdens were enhanced.

Doct<sup>r</sup> Rush

Bought of R. Hare & Son

1809	Jan.	1/2	bbl.	Table Beer	\$1.50
	Jan.	"	"	"	1.50
	Jan.	"	"	"	1.50
	Feb.	"	"	"	1.50
	Feb.	"	"	"	1.50

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For R. Hare & Son,  
Received Payment,  
W. Smith.

And it was doubtless at this time, while occupied in brewing and, of course, attending the stated meetings of the American Philosophical Society, that he addressed a letter to the Society on the tapping of air-tight casks by means "of a vent-peg and cock. The vent-peg is seldom firmly replaced and the consequence is the frequent souring or vapidness of vinous liquors. The quantity of liquor annually spoiled by the omission of the vent-pegs must be immense; and must be particularly great in those families where the tapsters are too numerous to be responsible for neglect," so he contrived a cock with two perforations "to obviate the necessity of a vent-peg." He submitted the communication "as an addition, though a small one, to the comfort and convenience of society at large—in any other light it can have no pretensions."



There were other activities in which he engaged to which it is now proper to give heed. Reference has been made to his occasional appearance in the columns of Philadelphia papers on subjects occupying men's thoughts in the first and second decades of the 19th Century, so that it seems opportune to pass in review some of the ideas set forth by Hare in a publication which appeared in 1810, bearing the title, "A Brief View of the Policy and Resources of the United States."

It must not be forgotten that at this time our infant Republic was drifting into alarming situations. The following abstracts are made from the text of the little volume:

"It is a universal observation of those, whose interests have been placed in opposition to companies, or large bodies of men, that these are very little actuated by generous passions. . . . Philanthropy is much more often assumed as an ornament, than excited by feeling. Religion and morality may lead us to deprecate the horrors which arise from national strife, but the recital of them, when we are not the immediate sufferers, rarely interferes with our slumbers—or interrupts the festivity of a meal. . . .

The greatest national right—the right of conquest—is the greatest moral wrong. Yet is this the boasted foundation of national sovereignty throughout the globe. . . . History does not furnish an instance of a nation, which has hesitated to seize an advantage—when encouraged by power—and invited by weakness—however destructive the consequences to the happiness of an injured nation—or inconsistent with the principles of justice or humanity."

"It is true that commerce and despotism can but ill subsist together;—but it is in the same way as the entertainment of the hedge-hog was incompatible with the comfort of the snakes.

The injury—the fear—and dislike, are not felt on the

side of the despot, but on that of commerce. Despotism has often destroyed commerce, because attended by an arbitrary and versatile policy, altogether inconsistent with those industrious pursuits which require permanency of duration, for the repayment of capital invested. But commerce never has—nor ever can subvert despotism; because, under circumstances so hostile to its prosperity—it can never make progress sufficient, to weigh against sovereign power.”

“Commerce was undoubtedly instrumental in the overthrow of the feudal system, but in this it rather aided—than opposed monarchical power. Seeking privileges and protection from the sovereign against the oppression of the nobles, the commercial towns in return, afforded him the means of preponderance over those turbulent vassals; but this was an operation, very different from that which would justify the idea of its competency—to subvert despotick power.”

“A magnanimous American would scorn to be dependent for the liberties of his country, on the duration of the balance of foreign power; but re-viewing our present means of resistance, compared with the force which we should be necessitated to oppose, there is no room for this noble independency of sentiment and he is forced, by the prospect of irresistible evil, to tolerate a predicament so monstrous, as that of being indebted for safety to those—who are permanent rivals in commerce—and consequently our enemies upon the principles of national conduct already laid down.

Could Great Britain and America be divested of those partial views of right, or interest, which are almost inseparable from human nature;—their mutual welfare—and even grandeur would be far from incompatible. The ocean and the land are not so confined in extent, as not to afford ample room for the greatest luxuriance in the prosperity of both countries. But by past experience we are taught, that it is in vain to hope



that two nations travelling on the same road to wealth, will ever proceed harmoniously; or find any other means of determining their respective claims, than that of power.

In respect to Great Britain however, it must be admitted, that although from justice or humanity she rarely abandons the course dictated by interest; yet in pursuing it, owing to the excellent form of her government and spirit of her laws, she has generally displayed more liberality than other nations. In the abstract, her policy may often be found too narrow, as it was in her treatment of this country while under her sway; but her conduct even in this respect was liberal, when compared with the colonial policy of France, Spain, Portugal, or Holland.

Had her system at an early period been as contracted as that pursued by those nations, we should never have had—the spirit—the liberty—the wealth—or the power—to which we owe our glorious independence. It was only through this superiour liberality or wisdom in construing her interest, that she ever permitted the extension of our commerce; for had she yielded to that jealousy and cupidity which it was so much calculated to excite—she would, at an early period, have depressed—or destroyed it.”

“Never was a comparison more fairly made in practice between opposite political systems, than we have seen in the trial of the policy of Washington, and that of Jefferson and his successor. The great founder of American independence saw the impossibility of a successful struggle for those commercial privileges—which America might in theory claim—but in practice could not establish—till time should afford her maritime strength. He saw the necessity of our rising under the wings of that very power—whose jealousy by our rivalry—we were destined sooner or later to excite. He saw that as yet in our political infancy—to contend for all our commercial rights—would cause the loss of every com-



mercial advantage and that early demonstrations of hostility, by alarming the fears of Great Britain, might give rise to a premature contest, and terminate not only the advantages we enjoyed from neutrality, but our rights as a commercial nation. In our imbecile state, he saw war could neither punish insult—nor retaliate injury; but would lead to a deprivation of that access to the ocean—which is essential to our wealth and glory. He was convinced of the folly of that boasted warfare of commercial restrictions which was proposed during his presidency by Madison, and which when since tried in practice—has proved more injurious to ourselves than to our enemies. He knew that as commercial intercourse could never have arisen without mutual advantage—it could not be interrupted without reciprocal injury.”

“Is not Great Britain quite as justifiable in using the ocean for her purposes—as we are for employing our immense territory for ours? We have had no other justification for taking it from the aborigines, than that they were too weak, too ignorant—or too unwise to defend it; and have not the British all these apologies for depriving us of the ocean? For although there be wisdom—knowledge—and strength in our country, adequate to justify a very different character—have we not to lament the total absence of these qualifications—in the actual conduct of the nation?”

“Considering all other nations as her natural foes, the true policy of America is to direct her whole energy to the creation of a power, adequate at some more favorable juncture—to elevate her above the evils of vassalage—or the fear of tyranny.”

“Though England can subsist without us, she is not insensible to the great advantages of an amicable intercourse, and so long as she is in dread of the growing power of her rival, she will be glad to purchase these benefits, by allowing us a commercial freedom, which her power enables her to

deny. To refuse those advantages which her fears or her necessities compel her to yield—because she will not grant us all, that in theory we might correctly demand, would evidently be impolitick—as on the other hand it would be disgraceful if we could look forward with indifference to the permanency of that degrading predicament, by which the extension of our commerce—is limited by its subserviency to her interest—and the duration of our repose—dependent on the continuation of her power.

Some Americans may exclaim, let us rather abandon the ocean, than enjoy such a partial, and degrading participation in maritime advantages. To me, however, it appears, that a total renunciation of the ocean, is the lowest degradation; and the utter impossibility of enforcing this abandonment in practice, has already been demonstrated. A portion of our countrymen are amphibious, and we might as well forbid the birds to fly, or the fishes to swim, as deny them access to their favourite element. Besides, a total renunciation, cuts off all hope of future, as well as of present commercial power; and should the command of the Atlantick ever fall into the power of any nation, on whom we should have no tie of interest, our seaboard might be frequently subjected to the inroads of hostility, and its horrid concomitants—plunder—and bloodshed.

By our situation, and by the genius of our government—a navy is our most effectual—and safest bulwark. It is the only engine of warfare, that can never aid in domestick oppression—always terrible to our enemies—and never dangerous to ourselves.

Were our shores unprotected by a navy, a large military force would in a state of warfare be requisite throughout the whole of our immense coast, to guard it from the sudden attack of the enemy. This would be no less oppressive in expense and far more dangerous to liberty.”



“The only obstacle to the creation of a navy—is the expense; but all history demonstrates, that no economy is so false—as that which leaves a nation defenceless. Governed by laws, which if they do not stimulate—are not injurious to industry—a prospect of wealth is open to us greater than has ever been displayed to a nation, if we be well defended against foreign oppression.”

“The debts contracted for this invaluable purpose, must inevitably be answered, by the prosperity they insure. Parsimonious views—would have checked our glorious revolution. The fear of bequeathing debt to posterity—is absurd. If we leave to them the power to defend their rights: and thus secure their future opulence, we provide eventually, ample means to answer every draught. But if we should bequeath them—imbecility—and hopeless vassalage; we leave to them a burden which nothing can relieve.”

“The aversion of the majority of our countrymen from national debt is our greatest obstacle. Familiar with the evils arising from insolvency, in any of the members of society;—by a false association, or analogy, they presume that the insolvency of a government, must be pregnant with consequences equally injurious to a nation. They are not aware that so long as the interest on publick debt is paid, insolvency in a government is only apparent. Nor do they see that credit is under some circumstances, equivalent to capital;—and that as much may be lost, by not employing credit—as by not occupying capital.”

“The following I imagine to be a simple, and obvious illustration of the primitive operation of credit, as a means of commercial interchange:—A raw material, being sold on credit, in lieu of remaining idle in the hands of the farmer, becomes in those of the manufacturer, an useful article; and he is enabled to return the farmer a better price, and to



furnish the merchant or consumer, a larger and cheaper supply, for home consumption or exportation. The same, or other merchants or manufacturers in the meantime, afford to the same or other farmers, the necessary articles for consumption, or implements for agriculture, which would have remained useless in their shops or stores, unless the parties at the outset, should have a sufficient command of some substantial medium of interchange, to make their respective purchases.

In the negotiation thus cited, each individual buys through the medium of his credit, and the several persons concerned, may have current accounts with each other, without any reference to money, unless as the received standard of value. In this case, therefore, the employment of credit, supercedes that of gold and silver, or any other substantial medium of interchange; and it may be considered as performing the office of such a medium, in a limited degree."

"Under a strict system of law, where the payment of debts is rigorously enforced, credit in that simple and primitive form in which it has just now been depicted, so far as it answers the purpose of a medium of interchange, is preferable to money."

"The manufacturing or trading stock, which had been preserved by the care, or exertion of the father; would in many cases be dissipated by the sloth or extravagance of the son;—and the frugal and industrious son, would no less often be deprived by the indolent and extravagant father, of that command of capital, which had been conferred on his ancestors,—but credit being in a great measure created by industry, skill and integrity—the possessor of these in every well regulated society, will have a greater or less command of such portions of the general stock or capital, as he can employ to so much greater advantage than the possessors, as to afford them a greater compensation for the loan of it, than they

could otherwise derive: provided, that his pretensions to credit be known to those, who may have the particular articles which it may be his interest to borrow, or their interest to lend."

"I trust it may be sufficiently plain from what I have advanced, that those who are endowed with mercantile credit, enjoy a valuable qualification or privilege in trade, when compared with those who have not this endowment."

"The various papers thus endowed with alienated credit, have been designated by the generick term—paper credit."

"Alienated credit may be no less current than coin, as in the case of bank checks or notes; or it may have a limited or sluggish currency, as in the case of mercantile notes, or bonds, bills or certificates."

"Among nations, in a mode in some degree similar, credit as a medium of commercial interchange, has the advantage, when compared with gold and silver money."

"Any great extension or diffusion of the advantages of credit demands a high degree of security from internal disturbance, or external dangers; and an improved state of trade, law, and morality."

"The difficulty attendant on the conception that paper credit should be comprised in an estimate of national capital, arises from the notion, that the debt itself is the object of valuation; whereas the real object of valuation, is the principle by which the debt is enabled to exist."

"An objection to credit as a medium of interchange, may be founded on its liability to depreciation in moments of alarm, arising from anarchy or invasion."

"The very active currency of bank checks and notes, is due to their superiority over gold and silver money, in conveniency of form and bulk."



“Bonds, bills, notes, bank stock, or national certificates, owe their more sluggish currency, to the payment of that interest, discount, or dividend, which renders it desirable or satisfactory to many individuals, to keep them in preference to money; as they afford equal security against eventual want, and are productive of a revenue to the holder.”

“Our publick debt may accumulate in a regular ratio, to the demand for banking or insurance capital; and in these states bank paper, as a circulating medium, obtains a decided preference over gold and silver; it follows, that, although our country is not rich in these metals, it is rich in an equivalent principle of wealth.”

“The alienation of bank credit in the form of notes, might be deemed a permanent sale, if these institutions were permanent; but as they are temporary, it must be deemed a lease during the period of their existence. For though their notes may be returned to one individual, they are immediately paid away to another; the quantity alienated, being on the whole nearly the same. Banks, therefore, may be considered as associations for creating and loaning credit.”

“Banks receive interest or discount for the loan of their credit. Governments receive capital or services in return for theirs, paying interest as I have already observed, for the difference between the currency of their stock, and the currency of money.”

“The alienation of the publick credit, should be considered as a permanent, and complete sale.”

“Enough has been said to demonstrate, that the poorer classes of society are very much gainers, if the capital obtained from wealthy citizens, or foreigners through the medium of the publick credit, be employed in the execution of designs worthy of its value. In any event, the poorer



classes can have no reason to complain, as they can never be called upon to pay more than that annual interest, which is so trifling when compared with the annual advantage, if the capital obtained by it, be invested in objects permanently beneficial. I say permanently, because it does not appear correct to employ the means afforded by credit, in defraying the ordinary expenses of government. This would in truth be a robbery of posterity; and in order to avoid a measure so replete with opprobrium, the publick credits should only be resorted to under circumstances, where the permanent character, or prosperity of the nation may be at stake."

"And shall Americans prefer a grovelling commercial inferiority—to a publick debt—the expected evils of which are proved to be imaginary; while the advantages may be equivalent—to national salvation—or to the difference between the degrading situation in which we now repose—and that glorious rank to which we should have been elevated—by the policy of Washington, and his coadjutors?"

"The only objection to borrowing, is the uncertainty of the issue of the trade, in which the loan may be invested.

But the United States may be considered as a trader, whose prosperous returns are mathematically certain, if through timidity or negligence, he does not refuse, or neglect the advantages, which are strewed in his path."

"The question then arises—will the chances of a wise and honest administration of affairs, be increased by extending to foreigners the privileges of voting? The honesty and ability of those who govern, must be determined by the degree in which virtue prevails over vice—and wisdom over folly—among those by whom they are chosen. If then in the United States the preponderance of virtue over vice, and of wisdom over folly—be sufficient, whence can arise any advantage, either to ourselves, or to foreigners, from admit-

“My objections to the present system of suffrage, are not founded on desire to deprive the mass of mankind of their inherent rights to self-government—but on a desire to secure objects of which the mass of society are competent to judge. When incapable of understanding the tendency of their suffrages, they cannot be said to enjoy their votes. They may vote for measures tending the very opposite of the consequences which they really wish.”

Business—contact with the great outer world—is responsible for the preceding digression into a field quite foreign to experimental science. But it is with a keen sense of pleasure that one turns to and reads the following communications addressed to a friend of similar tastes, thinking along the lines which quite early led to a most happy friendship.

I thank you for your account of your interesting experiments—Your apology for your delay in replying to my letter was unnecessary to me as I shall never suspect you of wilful neglect I can so fully understand the hurry of your experiments & other occupations—I am sorry I cannot bend my attention the way you encline it—I have during the past two years been occupied in the improvement of my casks & the

<sup>4</sup> Philadelphia. Printed by John C. Clark, No. 60 Dock St.



valves for closing them—It is astounding what a variety of modes of effecting the purpose of the common cock have been overlooked. I have been puzzled and perplexed by the variety that have offered themselves & in deciding between the various motives for preference presented by them. Without having become a mechanick I never could have succeeded—When I return again to rove unshackled in the path of experiment I shall come with new powers.

Your apparatus is very handsomely arranged—Some time when I have leisure I will send you a little improvement I have designed for keeping the frustrum in which the gases meet free from the ill consequences of the great heat—I wish you could combine the effects of the galvanic trough & of the compound blowpipe—I should like if possible to see the influences of Caloric & galvanic electricity united—Perhaps the heat would destroy the circuit—Would it be possible to give a shock from an electric battery to the earth, when exposed to caloric of this intensity—

To return to Business—I have not as yet sold any of my casks nor made up my mind to part with them—Yet it is probable I may find it [to] my interest to permit them to be employed for cyder as this may favour the demand for my porter in situations where the return of the casks would be difficult & expensive—The price is cheaper in proportion for large than small ones. A ten gallon one about eight dollars. The latter is about equal to the content of a gross of Porter Bottles.

I remain as ever

Professor Silliman

Very sincerely

Yale College

Your friend

New Haven."

ROB<sup>t</sup>. HARE."

Do not the words, "when I return again to rove unshackled in the path of experiment" tell the longing of this great exemplar of experimentation for the opportunity to



busy himself uninterruptedly with his beloved science? Imperative necessity had compelled him to become a man of business, but, while thus occupied, he thought upon the problems which had engaged Silliman and himself in earlier days, as well as upon more recent advances, *e.g.*, galvanism. There is also apparent an eye to advantage in the words:

“I may find it to my interest to permit them (the casks) to be employed for cyder as this may favour the demand for my porter. . . .”

From the next communication one may well conclude that “the important event” referred to was his marriage to Miss Harriett Clark, of Providence, R. I. This occurred on September 11, 1811. To this worthy couple were born in the course of time five sons and a daughter. Two of the sons died quite young. John Innes Clark Hare, the second child, studied chemistry under his father and subsequently abroad, but later entered upon the law, in which he met with signal success, becoming a professor in the Law School of the University and later a judge in the Courts of Philadelphia, where he was most highly esteemed and honored. Other sons, who lived into middle age, were Robert Harford Hare and George Harrison Hare. The daughter, Lydia, in due time, became Mrs. Frederick Prime, of New York City.

“Dear Silliman

“Philad<sup>a</sup>. Oct<sup>o</sup>. 16<sup>th</sup> 1811

I thank you for your kind letter congratulating me on an event which you justly deem the most important in this world—I regret you should still suffer from the unfortunate explosion of which you give me an account—Higgins you must remember suffered exactly in the same way from trusting that moisture would prevent the explosion.

The tightness of my casks was not accomplished without great difficulty—I was 18 months devising & experimenting ere I succeeded in making them perfect—My success however

is complete—I found on my arrival at New York six casks which had been sent there in the beginning of September in a high state of ripeness equalling the pressure of about three atmospheres—Although more than a month had elapsed not a drop of liquor appeared to have escaped & the ripeness had increased.

The heads I have latterly made of cast iron—The diameter of a barrel is reduced by 12 inches the length 42 inches. Hence the heads are light & do not cost more than from 60 to 75 cents each—being from  $\frac{1}{2}$  to  $\frac{3}{8}$  thick & concave on the external surface in a small degree forming an arch the versed sine of which is about  $\frac{1}{2}$  inch—The 120 bll. casks are about the same length & diameter  $8\frac{1}{2}$ —The bilge of both being raised very much.—The internal surface of the cask is heated as far as possible without ignition & soaked with melted beeswax while hot. The heads are covered with mastic dipped in eggs & quicklime in the same way as in luting & are then heated, covered with a film of wax & placed in the irons of the casks while warm enough to keep the wax completely in fusion—I used sometimes two pieces of plank luted together by white & red lead with lintseed oil & turned to that a portion of each went to form the tongue entering the irons of the cask—You tell me to send you a barrel of porter but don't say whether in bottles in the patent casks or common ones—If you wish a patent cask I must procure a friend to take on to you the instrument for drawing the liquor from the valve.

Pray inform me of your wishes—It will not be in my power to supply you with good bottled porter being out of my last winters stock—

I am as ever yours sincerely

ROB<sup>t</sup>. HARE."

"D<sup>r</sup>. Silliman

"Phila<sup>a</sup> March 19<sup>th</sup> 1812

I have only time to inform you that I have shipped your glass on board of the schooner Express Capt<sup>n</sup>. L. Hommedieu



—who has engaged to deliver them on Board a New Haven Packet for you when he arrives at New York—

Should you want a correspondent at Pittsburg who is conversant with Chemistry & chemical apparatus if you will write to Dr. Joel Lewis Jun<sup>r</sup>. & say you did so at my request you may be assur'd of a prompt & willing attention—Any money you may want paid you may refer to me for you know at Pittsburg a bill on Philadel<sup>a</sup>. is as good as cash—The trifle I have now paid is not worth attention till we have further dealings—

Yours as ever

ROB<sup>t</sup>. HARE.”

You may see my mechanical hands or the effects of them on this.”

(The allusion here is to a roughly written note and to blotches upon the paper.)

“Dr. Silliman

“Philad<sup>a</sup> April 5<sup>th</sup> 1812

I recd your letter respecting the gold a few days ago but have been too much occupied to make the necessary research until this day—Fortunately Lewis's Commercium is in my library & I have examined him—He is however so diffuse & unmethodical that I cannot convey his ideas on the subject of gold, without writing a short pamphlet—It will be necessary that the difficulty of your enquirer should be more precisely stated—& then I shall know if Lewis can aid him—I will however observe that he gives his opinion that solution in aqua regia, & precipitation by sulphat of iron is the only way of procuring gold in a state of purity—He mentions however cementing the metal in a very thin laminæ in a mixture of nitre or common salt & green vitriol with brick dust exposed to a strong heat & also exposing it to what he calls antimony—the sulphur of which destroys the baser metals while the antimony unites with the gold & is subsequently driven off by heat & oxydizement—I suppose he must mean a sulphuret of antimony—



I wish you would furnish an account of your late repetition of my experiments to the mineralogical Journal of New York or some other publication—Murray is the only European compiler that has condescended to notice them or to treat the earths as fusible matters. Bruce wrote to me for some acct. of them but though I refer'd him to my memoir with some directions he has not I believe noticed it as he professed himself desirous of doing.

Your friend called and paid the amt of the land carriage of your glass. The freight has not yet been paid—I have been too much occupied to pay him any attention—I have not dined at home these three weeks—I shewed him when he called some tubes of lead or tin I had been casting—I have rendered my apparatus so expeditious as to mould 25  $8\frac{1}{2}$  inch lengths in 11 minutes—

I am as ever

Yours

R. HARE.”

“P. S. Perhaps you had better send the account of your experiments to Nicholson as it may be neglected if it appears first on this side of the water—Original matter though of less intrinsick value generally receives more attention than that which has previously been printed—& especially if it has the misfortune first to shew itself here—”

Even in more modern times literature upon experimental subjects, if first printed in this country, received but little consideration from the editors of foreign journals. This custom must have irritated Hare very considerably, for it happened that in some European publications the description of his compound blowpipe and its extended uses were credited to a French savant.

On page 17 there is declared by Dr. Rush to Mr. George Clymer that the Trustees of the University would render a service to medical education by the introduction of a course on

natural philosophy into the curriculum. He further recommended that Robert Hare be placed in charge of the instruction. This idea seems at times to have seriously engaged the thoughts of the Trustees, for in 1812 they approached Hare upon the subject, receiving in reply to their advances the following letter:

“ To the Trustees of the  
University of Pennsylvania  
Gent<sup>n</sup>

A considerable period of time has intervened since I informed you that unless I could acquire some peculiar claims to notice I should despair of rendering the professorship of natural philosophy lucrative to myself or materially beneficial to the publick more especially as you had thought it necessary to restrict the lectures to objects not falling within the usual course of medical instruction as afforded in the university & to leave the question of attendance or non attendance to the option of the pupils.—With the view of acquiring some peculiar claims to attention I had made every arrangement for visiting Europe as soon as the settlement of the affairs of my late father should liberate me from the care of them & being disappointed in the sale of his estate that settlement was inevitably procrastinated however desired & sought for by me. Still however intent upon the improvement of the property while under my management I succeeded in an invention which promised to render my peculiar services highly important to the interests of my surviving parent & others concern'd. Then a new and unsurmountable duty arose in opposition to that prompt attention to the duties of my appointment which it was my ardent desire to afford & which you might reasonably have demanded. I did not however feel that there would be any necessity for my resigning the chair unless some one should appear capable & willing to perform the functions belonging to it especially



as I could not altogether give up the hope that I might eventually be enabled to perform them. In this predicament however I have at length found myself placed by the arrival of D<sup>r</sup> Patterson from Europe where I have understood that he has been engaged in making researches calculated peculiarly to qualify him as a lecturer in experimental science. I feel great regret when I review the impediments which thus oblige me to cede to another a situation for which my native propensities are so powerful. It is nevertheless pleasing to me to relinquish it under circumstances favorable to the interests of the medical school & to the merits of a juvenile candidate who actuated by the same taste as myself & more propitiously situated has already trodden over that preparatory ground which my judgment had pointed out.

I am Gent<sup>n</sup>

With due respect

Your ob<sup>t</sup> serv<sup>t</sup>

ROB<sup>t</sup>. HARE."

Manfully determined to carry out certain business plans, Hare continued in his customary way and waited. His disappointment was great, but his courage prevailed.

Hare was one of Washington's most "devoted political advocates, having always styled himself a *Washington Federalist*." On one occasion (1812) he embodied his sentiments of admiration in these verses:

Hail, glorious day, which gave Washington birth,  
To Columbia and liberty dear,  
When a guardian angel descended on earth  
To shed blessings o'er many a year.

Though heroes and statesmen, by glory enshrined  
May be seen in the temple of fame,  
No hero or statesman, unblemished we find,  
Save one, bearing Washington's name.

In the annals of war, many names are enrolled,  
 Of heroes who nations enslaved;  
 But have war's bloody annals of any one told,  
 Who a nation so nobly has saved?  
 Wealth, titles, and power, disdainfully spurn'd  
 Of heroes too often the aim;  
 From a king or his favors indignant he turn'd,  
 Only feeling his country's high claim.  
 To this ever true, in her trouble's dark night,  
 Intent on her welfare alone,  
 Against her proud tyrants, he urged the dread fight,  
 Till he forced them her freedom to own.  
 Next in France a strange demon uplifted its head,  
 All the nations of earth to betray,  
 And into its snares would Columbia have led,  
 Had not Washington warned her to stay.  
 Best and wisest of men! When counsell'd by thee,  
 Could thy people their treasure withhold?  
 When ruled by another, then could they agree  
 To lavish their millions untold?  
 By Genet insulted, by slander aggrieved,  
 If thy wrongs unrevenged could remain,  
 For rulers denouncing whom false he believed,  
 By a mob could thy Ligan be slain?  
 Can the voice of the country for whom he had bled,  
 E'er pardon a murder so base,  
 Or the tear-drops of millions, piously shed,  
 The deep stain from our annals efface?

“ Dr. Silliman

“ Philad<sup>a</sup> May 9<sup>th</sup> 1914

I have understood that through the liberality of Mr.  
 George Gibbs you have had an extensive collection of min-  
 erals added to your cabinet at Yale—Are they so arranged  
 as that a stranger may derive much benefit from them? At  
 what time do your lectures commence in this month—I recol-  
 lect you told me you entered on a course this month but I do



not remember the exact time. Mr<sup>s</sup>. H and myself have some thoughts of taking a ride which her health seems to require & desirous of combining intellectual with physical improvement we have turn'd our eyes towards New Haven. It would give me pleasure to converse with you on the late important discoveries & innovations of Sir Humphry Davy—I confess I admire him more as a practical than as a theoretical chemist. It seems my poor little discovery is doomed to meet mis-representation on every side. T. Cooper in a late number of the *Emporium* which he has taken from the incompetent hands of Coxe says that a degree of heat *nearly* equal to that of the burning glass may be produced by a blowpipe fed with the hydrogen & oxygen gasses. You may possibly recollect that he *stumpt* me when I was showing the experiments before Priestley by asserting that pure platinum had been fused in an air furnace. He now boasts as of a novelty of agglutinating the native grains—He has however rendered his work interesting & is proceeding on a plan of selection which I always thought the only one by which a periodical publication could be rendered really valuable, in this country.—

I hope this may find you & yours well & happy. I am  
as ever—

Yours

ROB<sup>t</sup> HARE.”

The “*Emporium*” referred to was the *Emporium of Arts and Sciences* conducted by Thomas Cooper, Esq., Professor of Chemistry and Natural Philosophy, Dickinson College, Carlisle, Pa. This journal was founded by Dr. John Redman Coxe, the fortunate competitor for the professorship in the University of Pennsylvania when the friends of Hare were advocating his claims and fitness for the same chair.

It was to Vol. 1, New Series, p. 180, that Hare directed Silliman's attention. In the preface occur these words from Cooper:

“ for what more useful work could the public desire, than one which should contain a judicious *selection* of practical papers on manufactures and the arts, from the more scarce and voluminous among the foreign publications, and a repository for original papers of the same description, furnished by men of research in our country? ”

“ Dear Silliman

“ Philad<sup>a</sup> March 30<sup>th</sup> 1815

I enclose you a letter from my friend W<sup>m</sup> Meredith, Esq. recommending a young gent<sup>n</sup> to your good offices who, he is desirous as you will see, should finish his education at Yale. The observations on the score of religion were drawn forth by my suggesting some doubt that it might interfere with your usual system to have one who is expected by his friends to adhere to Judaish belief under your more immediate care. I beg you will candidly state whether you are in the habit now of undertaking for a due compensation the care of young gent<sup>n</sup> so situated and whether there will be any objection on the grounds I have stated if on others there should be none—Meredith you know is a very devout Christian—The boy is I believe about fourteen—I presume W<sup>m</sup> Woodbridge led you to expect a visit from me ere now—I have indeed seriously intended it but in business depending on others there is always room for delay and rarely any room for shortening transactions—It is still however my hope and wish to visit you and I still believe I shall accomplish it—I have been constructing an improved blowpipe which I conceive will add to the facility of producing the most intense heat—

I propose to employ the Olefiant gas instead of Hydrogen & have no doubt the effect will be more powerful—Pray do you leave New Haven in the course of next month—After so long being delay’d in visiting Yale I should be very sorry to go there and find you away. When does your Mineralogical course begin—Do you think Accum of London is to be trusted to furnish any articles wanted in the chemical line.



The war has given the finishing blow to my business here and I shall I believe have to turn Lecturer by force of necessity—Coxe is universally complained of—Please to return an early answer concerning young Nathans as I shall probably soon set out for N. Y. and if I do not go to Yale immediately might send him by the steamboat in case you encourage me to do so— Your ever faithful friend

ROB<sup>t</sup> HARE."

For some reason Mr. Meredith was extremely interested in young Nathans. In his letter to Silliman he mentions that the lad was inclined toward the "profession of a Merchant" and says: "On the subject of Religion, I will only remark that Jews are educated *here*, at Harvard & at New York—& above all, at *Princeton* the reputed head quarters of Calvinism, etc.—Will Yale be more narrow than Nassau?"

The desire to reach the truth was ever a burning passion with Hare. Hence, it is easy to comprehend his thought in the following verses:

Oh, Truth! if man thy way could find,  
Not doomed to stray with error blind,  
How much more kind his fate!  
But wayward still, he seeks his bane,  
Nor can of foul delusion gain  
A knowledge till too late.

By sad experience slowly shown,  
Thy way at times though plainly known,  
Too late repays his care;  
While in thy garb dark Error leads,  
With best intent, to evil deeds  
The bigot to ensnare.

Is there a theme more highly fraught  
With matter for our serious thought  
Than this reflection sad,  
That millions err in different ways,  
Yet all their own impressions praise,  
Deeming all others bad?

To man it seems no standard's given,  
No scale of Truth hangs down from Heaven  
    Opinion to essay;  
Yet called upon to act and think  
How are we then to shun the brink  
    O'er which so many stray?

“ My dear Silliman

I should have written to you by mail some days ago of the unfortunate result of the election had I not felt too much out of spirits—Dorsey had nine Coxe only eight votes—One of my friends on whom the most explicit reliance was placed was induced to vote for Dorsey in consequence of the representation of D<sup>r</sup> Kuhn who left no stone unturned to defeat our hopes—He had been a patient of D<sup>r</sup> Physick this winter & they had become so intimate that the carriage of the latter was seen often to stand for hours before Kuhn's door.

This warped the old mans mind altogether to the side of Dorsey & he represented to the Trustees that if D<sup>r</sup> Coxe were incompetent in his present station he would be still more injurious in that to which it was proposed to remove him as the knowledge of materia medica is more important to a physician than a knowledge of chemistry— Had I been aware of these representations I might have refer'd to a letter written by Physick & Dorsey to the board some years ago requesting that the chair of materia medica should be merged in that of the institutes & practice & also have stated that those gent<sup>n</sup> had been quite willing Coxe should succeed provided I would be Dorsey's adjunct in Chemistry.

It is unfortunate I was lulled into so much security as I would have accepted that offer had I supposed there was any danger of the failure of D<sup>r</sup> C. . . . It is in contemplation to make another chair of chemistry in the Department of the Arts & my friends wish me to offer for it—Under any



other circumstances it would be worth nothing hardly but Coxe is so very unpopular that it will afford an opening probably for some advantages as I believe the trustees would all be glad to get rid of him.

I hope Lyman will not feel disappointed in consequence of any hopes I may have awaken'd—I write this in answer to yours of the 4th though I have not mention'd the receipt of it.

I sent your platinum by . . . Spring, Esq. brother to Binney. It is rather more than an eighth of what I purchased for 40 nearly, it must be nearly  $\frac{1}{2}$  lb—Cost therefore \$5—You need not send me this or my watch at present as I shall probably be nearer you ere long

Your friend

Ap, 1816.”

ROBT HARE.”

In this letter there is revealed a little of the politics which was taking place in medical and university circles. It will be recalled that in 1809, Dorsey and others were very desirous of having Hare succeed James Woodhouse. At this time there were those who, being hostile to John Redman Coxe and unwilling that he should have the Chair of Materia Medica, were ready, however, to place him in the Chair of Chemistry. There are records which plainly show that Coxe was not on very good terms with any of his colleagues; at times he was accused of encroaching upon the work of other Chairs. Indeed, even after he became the Professor of Chemistry, he busied himself to such a degree with the subject matter properly falling in other departments, that the Trustees were finally compelled to sever his connection with the institution. This, however, did not take place until about 1835. With the appointment of Coxe to the chair of chemistry, the majority then favored the appointment of Dorsey to the chair of materia medica; this prevented a vacancy

occurring in the chair of chemistry, which Hare and his friends had hoped might occur.

It was during the year 1816 that the friends of Robert Hare sought information from every possible source as to his fitness for the duties of such a position as they had in view. This may be gathered from letters sent them from persons not residing in Philadelphia. Thus to General Cadwalader, a trustee of the University, Dr. Jones of William and Mary College expressed himself as follows on March 22, 1816:

“It gives me great pleasure to reply to the inquiry contained in your favour of the 14th inst. as the subject is one upon which I can speak without hesitation.

Mr. Hare has distinguished himself not only by his knowledge of Chymistry, but by having made valuable contributions, both to the means, and the objects of chymical inquiry; and is in consequence advantageously known to the Chymists of Europe. I have ever regretted that other avocations had called his attention from a pursuit in which he had shown himself so eminently qualified to excel.

Should my opinion have any influence in promoting his appointment to the Chymical chair in the University of Pennsylvania, I shall felicitate myself on having promoted the interests of that Institution in particular, and the cause of science in general.

The mechanical skill possessed by Mr. Hare is an advantage of high importance, as it renders perfectly easy that which without it would frequently be relinquished as impossible. This advantage, as useful to the Institution as to the professor, is not likely to be obtained in any other candidate for the chair.”

And to John Hare Powel, a brother of Robert Hare, Henry Brevoort, Esq., of New York, addressed these lines on February 29, 1816:

“During my attendance of a course of lectures on Chym-



istry in the winter of 1813, delivered in the College of Edinburgh, Dr. Hope, the Professor, in describing the construction of your brother's blowpipe, mentioned his name in the terms following:

“ ‘ For the invention of this very ingenious machine, we are indebted to Mr. Robert Hare, jun., of Philadelphia; a gentleman whose merits claim a distinguished rank amongst the successful promoters of Chymistry, in the United States of America.’ ”

And, again, to General Cadwalader, Samuel L. Mitchell of Columbia University, wrote on March 23, 1815:

“ I have not answered your letter of the 14th inst. earlier, on account of a violent attack of the croop, which has incommoded me excessively. You honour me very much by asking my opinion concerning the qualifications of Mr. Hare to teach Chymistry as a Professor in the University of Pennsylvania. This gentleman has been known to me for ten years or more. I have perused some of the pages he has published on Chymical subjects. I have uniformly found him ardent in the pursuit of that kind of science. His actual attainments are of the high and respectable order, and he seems to be particularly qualified for devising and constructing experiments. It gives me pleasure to write you this opinion; and be assured, sir, of my service and respect.”

Despite these hearty endorsements the desired goal, as shown in Hare's letter of April, 1816, to Silliman, was not reached.

An inspection of old records in the University of Pennsylvania will disclose the greatest activity among medical men to preserve this position for men of their own particular profession.

“ Dear Silliman: “ New York March 20<sup>th</sup> 1817

About ten days ago I gave to Capt<sup>n</sup> Johnson of the schooner Encline nineteen pounds & a half of lead tubes which he undertook to deliver to you.

I have been much disgusted with the conduct of the people in an important district here for without conferring with me they enter'd into arrangements with an ignorant fellow of the name of Monell because he offered illumination at half price. I in consequence have concluded to let them manage it together. The undertaking is very laborious responsible & noisome—It is a fearful responsibility to have the eyes of the larger portion of the people of a city dependent on one for sight during many hours of the four & twenty. Unfortunately the greater part of the Corporation were luke warm or hostile to my undertaking so that after making a vast quantity of tubes burners it was not possible to use them. The funds which I had calculated to return into my hands through that medium were thus render'd useless & my operations too limited to admit of an economical use of fuel. My cylinder when properly fixed gave at the rate of 260 cubick feet of gas p hour. This is a prodigious quantity to be extricated in that time. In the coal gas process three times that quantity is a days work for a cylinder of the same dimensions—My beam an account of which will be shortly published is I think an admirable contrivance. It has at one end a circular arch head at the other a variable spiral curve arch so that the gasometer being hung at one end & a weight adequate to balance it at the other; this same weight will equiponderate with it at all points of its immersion. It is a plan simple, devoid of friction, easy of execution, & susceptible of correction at any time.

Did I ever mention that I tried an experiment with *two* electrical machines, last spring as I had proposed to do with *many*, in a previous letter to you. Cuthbertsons Electrometer was subjected successively to the action of two electrical machines—The effect of A was 2 that of B 5—The two machines were now put into action connected with each other & with the Electrometer according to my plan: That is the positive conductor of A communicating with the negative



conductor of B the positive conductor of this with the Electrometer. The effect on the latter was now equal to seven. The two machines were each separately but simultaneously connected with the Electrometer the effect was less than that of the stronger machine alone part of the excess, going off through the weaker one by a retrograde movement. You know my object was in this way to produce a mechanical electricity more nearly resembling the voltaic where the surface is divided into many plates of small area. This can in my opinion never be attained by the enlargement of a single machine any more than the pressure in the hydrostatick bellows can be increased by enlarging the pipe.

I have written to Brande of the Royal Institution giving an account of this project & experiment. I propose that a number of electrical cylinders shall be placed in a common frame & turned by an endless cord their positive & negative poles connected as above.—Does not the result I obtained by means of the two machines overset the doctrine of the existence of two fluids—Suppose we endeavour to explain the effect by that hypothesis—If I understand it the action of the machines determines the two fluids to their respective poles or conductors. In that case then the positive influence of the first & the negative influence of the second ought to neutralize each other meeting as they must in consequence of the connexion. Now by the other hypotheses the positive & negative states being relative not absolute that which it is negative in one view may well be positive in the other & hence the negative pole of the second instrument instead of being neutralized by the positive emission from the first may acquire a greater efficiency.

D<sup>r</sup> Clarke has been using us scurvily. I presume you must have read of his alledged discoveries by means of the heat evolved in the combustion of the gaseous elements of water. He does not condescend to notice your experiments

or mine. Brande acknowledges me as the Author but does not republish our experiments. I have written to him pointing out the injustice thus done us. I wish you would write to some of your correspondents on this subject. It appears that they have not succeeded in effecting the pretended decomposition of the metalloidal oxides in some attempts of the Royal Institution. They sneer at Clarke and say they knew that wonderful effects were produced by this means before by the accounts published in America. Do you recollect I propos'd to use the gases in state of mixture before emission?

Cooper has behav'd with his usual spirit of detraction in an article in Walsh's reviews. He very artfully contrives to transfer Clouds name to my blowpipe—I shall take him to task for it. Cloud never invented a blowpipe—If he invented anything it was a compound gas holder not a compound blowpipe. But it differ'd from one in my laboratory when at his request & in his presence I tried some experiments only in the following particulars. Mine was of wood & copper his of tin. The former had a flat receptacle for water the latter had a tall conical one. I leave it to you how far this was a wise alteration where equability of pressure was an object. He omitted various appendages which were not necessary to his purpose.

Pray assure Mr<sup>s</sup> Silliman of our grateful recollection of her attentions & the flattering partiality you have both so kindly expressed which we so sincerely reciprocate that we much regret that our abode is not likely to be nearer.

You must excuse me for scrawling—& believe your

Ever faithful friend

ROB<sup>t</sup> HARE."

On page 14 reference was made to the sturdy support shown Hare by Silliman in the days when the question of priority in regard to the compound blowpipe was raised. The following letter to the scientific public may, therefore, take its place here in its evident chronological order:



“Yale College, April 7, 1817.

“Various notices, more or less complete, chiefly copied from English newspapers, are now going the round of the public prints in this country, stating that “a new kind of fire” has been discovered in England, or, at least, new and heretofore unparalleled means of exciting heat, by which the gems, and all the most refractory substances in nature, are immediately melted, and even in various instances dissipated in vapour, or decomposed into their elements. The first glance at these statements, (which, as regards the effects, I have no doubt are substantially true,) was sufficient to satisfy me, that the basis of these discoveries was laid by an American discovery, made by Mr. Robert Hare, of Philadelphia, in 1801. In December of that year, Mr. Hare communicated to the Chemical Society of Philadelphia his discovery of a method of burning oxygen and hydrogen gases in a united stream, so as to produce a very intense heat.

In 1802, he published a detailed memoir on the subject, with an engraving of his apparatus, and he recited the effects of his instrument; some of which, in the degree of heat produced, surpassed any thing before known.

In 1802 and 1803, I was occupied with him, in Philadelphia, in prosecuting similar experiments on a more extended scale; and a communication on the subject was made to the Philosophical Society of Philadelphia. The memoir is printed in their transactions; and Mr. Hare’s original memoir was reprinted in the *Annals of Chemistry*, in Paris, and in the *Philosophical Magazine* in London.

Mr. Murray, in his *System of Chemistry*, has mentioned Mr. Hare’s results in the fusion of several of the earths, etc., and has given him credit for his discovery.

In one instance, while in Europe, in 1806, at a public lecture, I saw some of them exhibited by a celebrated Professor, who mentioned Mr. Hare as reputed author of the invention.

In December, 1811, I instituted an extended course of experiments with Mr. Hare's blowpipe, in which I melted lime and magnesia, and a long list of the most refractory minerals, gems, and others, the greater part of which had never been melted before, and I supposed that I had decomposed lime, barytes, strontites, and magnesia, evolving their metallic bases, which burnt in the air as fast as produced. I communicated a detailed account of my experiments to the Connecticut Academy of Arts and Sciences, who published it in their Transactions for 1812; with their leave it was communicated to Dr. Bruce's mineralogical Journal, and it was printed in the 4th number of that work. Hundreds of my pupils can testify that Mr. Hare's splendid experiments, and many others performed with his blowpipe, fed by oxygen and hydrogen gases, have been for years past annually exhibited, in my public courses of chemistry in Yale College, and that the fusion and volatilization of platina, and the combustion of that metal, and of gold and silver, and of many other metals; that the fusion of the earths, of rock crystal, of gun flint, of the corundum gems, and many other very refractory substances; and the production of light beyond the brightness of the sun, have been familiar experiments in my laboratory. I have uniformly given Mr. Hare the full credit of the invention, although my researches, with his instrument, had been pushed farther than his own, and a good many new results added.

It is therefore with no small surprise that, in the *Annales de Chimie et de Physique*, for September, 1816, I found a translation of a very elaborate memoir, from a Scientific Journal, published at the Royal Institution, in London, in which a full account is given of a very interesting series of experiments, performed by means of Mr. Hare's instrument; or rather one somewhat differently arranged, but depending on the same principle. Mr. Hare's invention is slightly men-



tioned in a note, but no mention is made of his experiments, or of mine.

On a comparison of the memoir, in question with Mr. Hare's and with my own, I find that very many of the results are identical, and all the new ones are derived directly from Mr. Hare's invention, with the following differences—In Mr. Hare's, the two gases were in distinct reservoirs, to prevent explosion; they were propelled by the pressure of a column of water, and were made to mingle, just before their exit, at a common orifice. In the English apparatus, the gases are both in one reservoir, and they are propelled by their own elasticity, after condensation, by a syringe.

Professor Clarke, of Cambridge University, the celebrated traveller, is the author of the memoir in question; and we must presume that he was ignorant of what had been done by Mr. Hare and myself, or he would candidly have adverted to the facts.

It is proper that the public should know that Mr. Hare was the author of the invention, by means of which, in Europe, they are now performing the most brilliant and beautiful experiments; and that there are very few of these results hitherto obtained there, by the use of it, (and the publication of which has there excited great interest,) which were not, several years ago, anticipated here, either by Mr. Hare or by myself.

As I have cited only printed documents, or the testimony of living witnesses, I trust the public will not consider this communication as indelicate, or arrogant, but simply a matter of justice to the interests of American science, and particularly to Mr. Hare.

BENJAMIN SILLIMAN."

The devoted student of science now gave up his Philadelphia residence. There is no knowledge of the reason for this step, unless, perhaps, the abandonment of business and

the uncertainty as to his future occupation. It is also possible that financial straits prompted him to turn to Providence from which point he next writes to his dear Silliman.

“ My dear Silliman:                      “ Providence April 23<sup>d</sup> 1817

On my arrival here I found your letter enclosing eleven dolls—Having bid a permanent adieu to New York & having at present no views elsewhere this place will probably for some time be my home. Of course I will thank you to send hither the Pamphlets & any communications for me—

I left the enclosures for the European philosophers at M<sup>r</sup> Eastburns (reading rooms) whose brother will go to Europe shortly & M<sup>r</sup> E offer'd to put into his hands any thing we destined for that part of the world—I need not say I was pleased with your letter in the *Courier*—If the rest of the world estimated my humble pretensions as you do I should stand higher than I deserve—It appears to me however that the most efficient step is yet to be taken though a very simple one which is to solicit a republication of your memoir in one of the London Journals—You may possibly remember that I suggested this measure when it was first transmitted me though you modestly satisfied yourself with publishing it in the mineralogical journal & letting it take its course.—It really seems bad policy to publish any thing in this Country upon Science especially in the first instance—It is rarely attended to in England & we are so low in capacity at home that few appreciate any thing which is done here unless it is sanction'd abroad. A very sensible young friend of mine who had been in England speaking of my pamphlet told me as a ground of exultation that it was very near being reviewed in the *Edinburgh Review*—a prodigious honour to be sure—There is nothing I am now satisfied in which there is more intrigue or charlatanism than in the business of literary or scientifick reputation.—Tilloch having republished my



memoir will probably be willing to republish yours—If you think proper send it to him yourself or if you prefer it I will—M<sup>r</sup> Eastburn will no doubt despatch it by his brother should you send it to his care—He is very fond of appearing in that sort of business & is apparently a very amiable man & disposed to oblige—

You will see a cut of my beam probably in the next N<sup>o</sup> of the ——— Magazine by Moses Thomas Phila<sup>a</sup> & also a copy of my letter to Brande on the electrical experiment—

When I was in Philad<sup>a</sup> my friends suggested the idea of my opening a chemical & Drug store as the most eminent of the Physicians would give their prescriptions—& it would be a great help in any views on the chemical chair in any future vacancy—There is something attractive in the idea but I know by experience that personal motives do little for men in business in the long run & I am not qualified well for minute economy or minute attention—It was propos'd I should associate myself with some one who should understand & transact the minutia but such dependency is dangerous—Thinking, however on the subject—Your pupil Lyman Foot came into my mind as one with whom such a connection might be safe & in some respects advantageous should it so fall out as to be desirable to both—I cannot aver however that I have any very serious inclination for the plan but mention it to you that you may say what occurs to you in any moment of leisure—

With the kindest salutation to you & yours & begging pardon for this hasty scrawl—

I remain your faithful friend

1817

ROB<sup>t</sup> HARE."

In some unrecorded way Hare was interrupted in his wanderings and placed in a teaching position in the College of William and Mary, Virginia. The minutes of that institution bear record that "Dr. Robert Hare appeared before

the Faculty and qualified as professor of Natural Philosophy and Chemistry, February 25, 1818. He attended several faculty meetings, March 14 being his last, after which his name does not appear in the record. During this short interval from February 25 to March 14, he appears to have gotten into trouble with the students, attending his lectures, by charging a fee, which they condemned. The Faculty upheld Dr. Hare, and upwards of twenty-five were dismissed."

These facts were recently (May 4, 1916) communicated by President Tyler of the College of William and Mary to the writer. He in turn submits with much pleasure the following testimonials from persons conversant with Dr. Hare's brief career in the Virginia College.

"Having been a regular attendant on the Chymical Lectures of Dr. Hare this spring, I am enabled to say, that speaking extempore with little time for preparation, he has satisfactorily explained the principles of Chymistry, and illustrated them by a great number of experiments, in the exhibition of which he has been very successful, and discovered much ingenuity and manual dexterity.

As a lecturer, Dr. Hare possesses advantages which deserve particular notice. From his great experience in Chymical pursuits, he has never appeared to be at a loss in expounding the most difficult phenomena which the science presents; and by the force of a talent which seems peculiar to himself, he is enabled to attract and rivet the attention in discussing the most ordinary and familiar topics. His success in the manipulations, in my opinion, is not owing more to the care with which he selects his agents, than to the mechanical skill with which he prepares his apparatus, or supplies it entirely where it is found wanting.

In fine, Dr. Hare is most enthusiastically devoted to his profession, and it is obvious to all who attend to the character of his pursuits when not engaged in the exercises of



the College, that he possesses a genuine love for philosophical inquiry, and that he regards science almost exclusively as the business of his life.

FERDINAND S. CAMPBELL."

Williamsburgh, 20th May, 1818."

"I have attended many of the Lectures of Dr. Hare, during the course, in the College of William and Mary, and found his experiments, and the explanation of them very satisfactory. I remember no instance in which his experiments did not succeed.

ROBERT NELSON."

Williamsburgh, 23d May, 1818."

"I take great pleasure in stating, that in the course of Lectures which you have delivered here on Chymistry, you have evinced, as far as I am able to judge, great acquirements in that science, and have certainly used exertions almost unparalleled, amidst difficulties the most perplexing and harassing. Very respectfully,

Your obedient servant,

J. AUG. SMITH."

William and Mary College, May 20, 1818."

"We whose names are hereunto subscribed, do certify upon a review of the course of Chymical Lectures delivered by Dr. Hare, during the present session in this institution, that he has explained the principles of Chymistry to his class, with perspicuity and ability, and well adapted for the purpose of elucidating the subject for which they were introduced. We moreover acknowledge, and take this mode of expressing our thanks, that we are indebted to him, not only for the fidelity with which he has discharged his duties in the lecture room, but more especially for his laborious and unparalleled exertions which he made for the class in the laboratory, in renovating and preparing the apparatus so as

to ensure as far as possible the success of the experiments and to enlarge their sphere.

Signed, etc.

Fifty Students."

Williamsburgh, June 2d, 1818."

"We the undersigned members of Dr. Hare's classes, understanding that a report has reached Philadelphia, and is there circulated, in which it is stated, that Dr. Hare is disliked by, and is unpopular with those who attend his Lectures, take this means of rectifying any evil impressions, which such report may have caused, and of testifying our respect and esteem for him as a gentleman, and the high opinion we entertain of his abilities as a professor.

June 17th, 1818."

Signed, etc."

Turning for a moment from these expressions of Hare's skill and ability be it said that in 1818 John Redman Coxe resigned his chair of chemistry in the University of Pennsylvania. Immediately numerous candidates for it appeared, bringing most powerful influence to bear upon the Trustees. Here is not the place to review the acts of the clashing interests. Since then one hundred years have passed and one's judgment now is surely disinterested enough to declare that the medical faculty seemed bent upon objecting to any one not trained in a medical school and not holding the medical doctorate. Not a word derogatory of Hare's character or ability as a chemist was uttered by his opponents. However, said they, not having been medically trained he was unfit. Some of the candidates had done absolutely nothing in chemistry. Their supporters argued that chemistry was a subject which could quite easily be acquired from text-books—at least, sufficient of it to qualify them for their medical pursuits.

Among the many applicants for consideration was



Thomas Cooper—a most remarkable character who had come out from England with Priestley. Those who have followed his career will read his petition to the Trustees with keen interest and zest.

“ August 4 1818

“ To the Trustees of the  
University of Pennsylvania  
“ Gentlemen,

There being a vacancy in the chemical chair of this University, I beg leave to be considered as a candidate: and as the other candidates exhibit to the Trustees their claims to the appointment, it seems proper that I should communicate mine.

It is well known to the members who compose the board of Trustees, that I have long been devoted to the study and pursuit of chemistry. For seven years past, I have been a public lecturer in chemistry; and I may fairly presume that my reputation in that character, was the chief reason for appointing me to the professorship I now hold in the Faculty of Arts in this University; and that I stand in no need of testimonials from persons of less experience than myself.

I have also a right to say, that I have laboured more to promote chemical science in this country than any other man in it, having been a longer time devoted to this study, not only as a lecturer, but an author. As the Trustees have done me the honour to accept eight volumes in 8vo. of my chemical publications, they have the means of judging of my deserts in this respect.

I find from a collection of certificates and testimonials published by Mr. Robert Hare, that Dr. Chapman was of opinion in his case, that it was by no means necessary for a professor of chemistry in the faculty of medicine, to be also a regular Physician; it appearing from Dr. Chapman's statement, that *Davy* of London, *Murray* of Edinburgh, and

*Vauquelin* of Paris, had not studied or graduated as Physicians. If not *necessary*, however, it is highly *expedient*, that your chemical professor should be not only a Doctor in medicine nominally, without practice, but also a Physician by practice, inasmuch as it is his duty to pass upon the qualifications of students who apply for medical degrees. Whether the other candidates are practising physicians, must be known to the Board: my own claims to that title are as follow:

In London, I attended the Anatomical Lectures of Mr. Sheldon of Great Queen Street. I attended also a clinical course at the Middlesex Hospital. I attended at my leisure hours, the patients of Dr. Ferriar of Manchester under his direction. I have practised openly and avowedly as a Physician in this country, for a longer time than any present member of the Medical Faculty of this University.

I exhibited to many of the Trustees on a former application, after the death of Dr. Barton, the testimonials of Judge Walker, of Judge Brackenridge, of the Rev. Mr. Campbell of Carlisle, of Dr. Armstrong and Dr. Gustine of the same place, that I have continuedly practised as a physician, regularly and repeatedly employed in their families. Those written testimonies employed in my behalf, on that occasion, are now dispersed and mislaid. But I may appeal to Judge Duncan for my having practised regularly as a physician in the county of Northumberland, uniformly called in by the resident physicians there, for twenty years past, at every consultation: in particular, that I have repeatedly attended his sister, and her family, the wife of Judge Walker, and been repeatedly consulted by him, by letter, since we have resided at different places. I appeal to Judge Duncan and Judge Gibson for full and satisfactory testimony, that at Carlisle I was regularly called in upon every occasion of difficulty by the Physicians of that place. That I have repeatedly attended



in that capacity the families of those two Judges, and of Judge Brackenridge; and that Dr. Armstrong, Dr. Gustine, and Dr. Foulke, of Carlisle, each of them confided their wives when sick to my medical direction. I name these gentlemen (Judge Duncan and Judge Gibson) because I know that some of the Trustees have applied to them, to ascertain the truth of this general statement; and I have appealed to their testimony as to these points, while they were in the city: and without any knowledge of the answers they may have given, I rest upon the evidence they have afforded in reply to such enquiries: and as those gentlemen are here so frequently, and so many opportunities occur to the members of this Board who are also members of the Bar, to verify this statement, I make it in full confidence of the result.

Having successfully attended the Rev. Mr. Campbell to whose episcopal congregation I belonged, in a long and dangerous illness—and as I am informed by him that he had written on a former occasion to a reverend gentleman, a Trustee of this University, on the subject of my general character there, as well as my medical talents, I refer to that letter in support of this statement; not thinking it necessary to multiply proofs of the good opinion of my friends. I may also mention, that to some of the Trustees here, it is known (as I have reason to believe) that the notes and letters of Dr. Wistar to me were addressed to Dr. Cooper: the instances I had preserved to this style of direction, are lost with my other documents formerly shewn. I have also been honoured with the Degree of Doctor of Medicine by the University of New York, on the motion, as I understand, of Drs. M'Nevin and Hossack, gentlemen sufficiently competent to speak of my title to the distinction thus conferred.

And finally, at their last session, the Medical Society of this city, without my knowledge appointed me to deliver the annual report of the progress of *Materia Medica* for the

past year: which I did, as I have reason to believe, to the full satisfaction of that body.

In one respect only, it may be argued that I am not a regular Physician; though my education, my course of reading and study, and long practice, entitle me to that character, as fully as any other medical gentleman of this city. Having, at the direction of my father, pursued the study of the Law as my Profession, I have always deemed it improper to take fees for my attendance as a Physician, and to act for profit in a double capacity. Judge Walker could speak to this point from his own knowledge, having experienced and known my repeated refusals in his own case as well as others for at least twenty years past.

I therefore have a just right to be considered as a Physician, not only by formal title honourably acquired, but by a regular course of study, by long experience, and extensive practice; and the objection formerly made to Mr. Hare in this respect, did not then, and does not now apply to me. My short residence in Philadelphia renders this necessary.

Should I succeed in this Application for the vacant Chair of Chemistry, I shall endeavour to justify the preference so given in my favour, by assiduity in pursuit of the duties of my department.

I have the honour to be,

Gentlemen,

Your obedient servant,

THOMAS COOPER, M.D.

282 Chestnut Street."

The Trustees, however, had taken particular care to inform themselves as to the real qualifications of the gentlemen who offered themselves for this most important post and wisely selected that one whose labors live to-day, while most of his rivals and their works have passed into oblivion.

It must have been a happy moment when Robert Hare indited the following note of acceptance to the Board:



"Dear Sir:

"Sep<sup>t</sup> 3<sup>rd</sup> 1818

I have rec'd your letter enclosing a copy of part of the minutes of the Trustees of the University of Pennsylvania by which it appears that I am appointed by them professor of chemistry in the medical department of that Institution. In reply I beg leave through you to inform them of my grateful acceptance of this appointment.

Tendering you my acknowledgments for your kind congratulations

I am Sir

With sincere regard

Very truly yours

Ew<sup>d</sup> Fox Esq<sup>r</sup> "

ROB<sup>t</sup> HARE."

The friends of Hare were, indeed, happy over his success. They knew him and his absolute fitness for the position and were particularly glad that he was now to have the opportunity of exercising his splendid talents. Throughout this country there was deep satisfaction.

Harvard in 1816 had expressed its high regard for Hare's achievements by the bestowal of the Medical doctorate upon him.

Another striking incident was that after Hare had been honored with election to the chair, Dr. Cooper promptly delivered himself of an address, November 5, 1818, to the Medical Faculty on the connection of medicine and chemistry. The following quotations are characteristic:

"During the late discussion previous to the election of Dr. Hare to the Chair of Chemistry in the Faculty of Medicine of this University, two opinions appear to have been advanced by the medical faculty: 1st. That the Chair of Chemistry ought not, or at least need not, be filled by a medical character; because the chair of chemistry was not necessary to, and ought to be separated from, the faculty of medicine" . . . "This appears to have been the general

sentiment of the medical faculty of the University of Pennsylvania; for having applied to Dr. Hare, they persuaded him to relinquish his privilege and his duty, of passing upon the qualifications of the medical students when they came forward to be examined for a degree, and of signing their diplomas; confining himself simply to the examination of the students—in chemistry only—the rest of the faculty, reserving to themselves the exclusive right of deciding upon the result of such examination, which was to take place in their presence. To this proposal it was understood, and indeed announced, that Dr. Hare had assented. Whether the Trustees of the Institution will assent to it also, time only can shew.

This general opinion of the inutility of chemistry to medicine was not confined to the medical faculty in the University.” . . .

“ In this state of things, I deemed it an allowable use of my situation as Professor of chemistry in the Faculty of the Arts, to shew, that there is a connexion between medicine and chemistry, and to trace an outline of that connexion. It appeared to me, that heresy in question ought to be combatted by some one, and I found no one likely to do it, if I did not.”

The next letter was surely written in Philadelphia—in the old home—to which Hare had returned for the purpose of assuming at last the responsible duties of the Chair of Chemistry in the Medical School of the University of Pennsylvania. In his mind were floating problems to which he must have felt he could now give his best effort. He was eager for the fray.

“ Dr S——

“ Sep<sup>t</sup> 11<sup>th</sup>, 1818.

I forgot to put you in mind of the kelp which was to be sent you by Guy Lussac & of which you were to let me have some—

Your f<sup>d</sup>

ROB<sup>t</sup> HARE.”



“ You will probably soon be on a visit to New York— If so let me hear of it—I wrote to M<sup>r</sup>. Whitney for some gun barrels etc. but have no answer. Is (he) at New Haven? Should you see him ask if he got my letter—

Have you varied your mode of obtaining potassium? Have you tried Tenants plan?

I should like to have a furnace constructed after your plan if it could be done without giving yourself trouble. You mention’d there were some workmen who had made yours who would make others to order—Please therefore order one for me agreeably to your own fancy.”

It is difficult to separate one’s self from this part of Hare’s life without giving expression to a few thoughts. All interested in research and possessed of the spirit of this master will certainly feel that it was a supreme moment to him. He was now thirty-seven years old. At twenty he had arrested the world’s attention by his discovery of the compound blow-pipe and its concomitants. Compelled by circumstances to give himself to a manufacturing business—meant almost complete divorcement from his scientific work; but recall how, despite the strain upon him, he improved the blowpipe, and read, thought and advised with his faithful friend, Silliman, upon other attractive problems. He was familiar with the advances of European chemists and must have chafed under his restraint, but at no time is any impatience displayed; on the contrary, there was a quiet waiting for the day of freedom, when he could more completely follow his specialty. That time had now arrived and it is easy to imagine his eagerness to begin and set in order his workshop.

Had Hare made no other contribution to science than that involved in the oxyhydrogen flame, he would be worthy of highest praise. But he did vastly more and we will now follow him through a long period of brilliant experimentation with the certainty of coming from it and marvelling at his splendid successes.

## SECOND PERIOD

1818-1847

IN Europe, early in the 19th Century, Davy isolated, with the help of voltaic electricity, sodium and potassium from their hydroxides; Dalton's chemical atomic theory was the subject of constant discussion; Berzelius announced his electrochemical doctrine and was issuing his exhaustive study of atomic and molecular weights, while in 1819 Du Long and Petit printed their interesting observations on atomic heat.

Robert Hare read and studied the current scientific literature, and he was conversant with all these discoveries, and dwelt, at least in thought, upon them. Hence, the influence which they exercised upon him will be apparent as he enters upon his new, unembarrassed period of experimentation. Hare took up the duties of his chair energetically and with great enthusiasm, and before long important communications were coming from his pen. The direction of his research work should be studied and followed with unusual care, for it cannot fail to interest the scientist. In letters sent by him to Silliman there are indications that the voltaic current had long been the subject under his consideration.

In Silliman's comments upon James Woodhouse it was mentioned that the latter, upon his return from England, brought with him a Cruikshank trough—a modification of the voltaic cell or pile,—and that possession of this novelty added to the repute of Woodhouse among his contemporaries. This trough, no doubt, had been seen by both Silliman and Hare. To the latter it appealed. He at once recognized its superiority over the original of Volta. Then, too, the mysterious current was to him a constant source of interest, so that inquiries, on his part, followed as a matter of course. The re-



markable achievements made possible by electricity had inspired him to give time and thought to its further application.

Cognizant of the sources of the "subtile agent" and probably conscious of their several defects he offered promptly, upon the assumption of his professorial duties in 1818, a new theory of galvanism with a description of the *calorimotor*, a new galvanic instrument. This contribution, now a century old, must have been the consequence of much quiet study and labor in the years when business so completely absorbed his time. It is such a splendid document and the *calorimotor* marks such a decided epoch in electro-chemistry that it seems best to let the distinguished experimenter speak for himself:

"I have for some time been of opinion that the principle extricated by the Voltaic pile is a compound of caloric and electricity, both being original and collateral products of Galvanic action.

It is well known that heat is liberated by the Voltaic apparatus, in a manner and degree which has not been imitated by means of mechanical electricity; and that the latter, while it strikes at a greater distance, and pervades conductors with much greater speed, can with difficulty be made to effect the slightest decompositions. Wollaston, it is true, decomposed water by means of it; but the experiment was performed of necessity on a scale too minute to permit of his ascertaining whether there were any divellent polar attractions exercised towards the atoms, as in the case of the pile. The result was probably caused by mechanical concussion, or that process by which the particles of matter are dispersed when a battery is discharged through them. The opinion of Dr. Thomson, that the fluid of the pile is in quantity greater, in intensity less, than that evolved by the machine, is very inconsistent with the experiments of the chemist above mentioned, who before he could effect the separation of the elements of water by mechanical electricity, was obliged to confine its emission

to a point imperceptible to the naked eye. If already so highly intense, wherefore the necessity of a further concentration. Besides, were the distinction made by Dr. Thomson correct, the more concentrated fluid generated by a galvanic apparatus of a great many small pairs, ought most to resemble that of the ordinary electricity; but the opposite is the case. The ignition produced by a few large Galvanic plates, where the intensity is of course low, is a result most analogous to the chemical effects of a common electrical battery. According to my view, caloric and electricity may be distinguished by the following characteristics. The former permeates all matter more or less, though with very different degrees of facility. It radiates through air, with immeasurable celerity, and distributing itself in the interior of bodies, communicates a reciprocally repellent power to atoms, but not to masses. Electricity does not radiate in or through any matter; and while it pervades some bodies, as metals, with almost infinite velocity; by others, it is so far from being conducted, that it can only pass through them by a fracture or perforation. Distributing itself over surfaces only, it causes repulsion between masses, but not between the particles of the same mass. The disposition of the last-mentioned principle to get off by neighbouring conductors, and of the other to combine with the adjoining matter, or to escape by radiation, would prevent them from being collected at the positive pole, if not in combination with each other. Were it not for a modification of their properties, consequent to some such union, they could not, in piles of thousands of pairs, be carried forward through the open air and moisture; the one so well calculated to conduct away electricity, the other so favourable to the radiation of caloric.

Pure electricity does not expand the slips of gold-leaf, between which it causes repulsion, nor does caloric cause any repulsion in the ignited masses which it expands. But as the



compound fluid extricated by Galvanic action, which I shall call electro-caloric, distributes itself through the interior of bodies, and is evidently productive of corpuscular repulsion, it is in this respect more allied to caloric than to electricity.

It is true, that when common electricity causes the de-flagration of metals, as by the discharge of a Leyden jar, it must be supposed to insinuate itself within them, and cause a reaction between their particles, but in this case, agreeable to my hypothesis, the electric fluid combines with the patent caloric previously existing there, and, adding to its repulsive agency, causes it to overpower cohesion.<sup>1</sup>

Sir Humphry Davy was so much at a loss to account for the continued ignition of wire at the poles of a Voltaic apparatus, that he considers it an objection to the materiality of heat; since the wire could not be imagined to contain sufficient caloric to keep up the emission of this principle for an unlimited time . . . But if we conceive an accumulation of heat to accompany that of electricity throughout the series, and to be propagated from one end to the other, the explanation of the phenomenon in question is attended by no difficulty.

The effect of the Galvanic fluid on charcoal is very consistent with my views, since next to metals, it is one of the best conductors of electricity, and the worst of heat, and would therefore arrest the last, and allow the other to pass on. Though peculiarly liable to intense ignition, when exposed between the poles of the Voltaic apparatus, it seems to me it does not display this characteristic with common electricity. According to Sir Humphry Davy, when in connexion with the positive pole the latter is less heated than

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<sup>1</sup> Possibly the electric fluid causes decompositions when emitted from an impalpable point (as in the experiments of Wollaston) because its repulsive agency is concentrated between integral atoms, in a mode analogous to that here referred to; a filament of water in the one case, and of wire in the other, being the medium of discharge.

when, with respect to the poles, the situation of the wire and charcoal is reversed. The rationale is obvious: Charcoal, being a bad conductor, and a good radiator, prevents the greater part of the heat from reaching the platina, when placed between it and the source whence the heat flows."

"I had observed that as the number of pairs in Volta's pile had been extended, and their size and the energy of the interposed agents lessened, the ratio of the electrical effects to those of heat had increased; till in DeLuc's column they had become completely predominant; and, on the other hand, when the pairs were made larger and fewer (as in Children's apparatus) the calorific influence had gained the ascendancy. I was led to go farther in this way, and to examine whether one pair of plates of enormous size, or what might be equivalent thereto, would not exhibit heat more purely, and demonstrate it, equally with the electric fluid, a primary product of Galvanic combinations. The elementary battery of Wollaston, though productive of an evanescent ignition, was too minute to allow him to make the observations which I had in view.

Twenty copper and twenty zinc plates, about nineteen inches square, were supported vertically in a frame, the different metals alternating at one half inch distance from each other. All the plates of the same kind of metal were soldered to a common slip, so that each set of homogeneous plates formed one continuous metallic superficies. When the copper and zinc surfaces, thus formed, are united by an intervening wire, and the whole immersed in an acid, or acetosaline solution, in a vessel devoid of partitions, the wire becomes intensely ignited; and when hydrogen is liberated it usually takes fire, producing a very beautiful undulating, or corruscating flame.

I am confident, that if Volta and the other investigators of Galvanism, instead of multiplying the pairs of Galvanic plates, had sought to increase the effect by enlarging one



pair as I have done, (for I consider the copper and zinc surfaces as reduced to two by the connexion) the apparatus would have been considered as presenting a new mode of evolving heat, as a primary effect independently of electrical influence. There is no other indication of electricity when wires from the two surfaces touch the tongue, than a slight taste, such as is excited by small pieces of zinc and silver laid on it and under it, and brought into contact with each other.

It was with a view of examining the effects of the proximity and alteration in the heterogeneous plates that I had them cut into separate squares. By having them thus divided, I have been enabled to ascertain that when all of one kind of metal are ranged on one side of the frame, and all of the other kind on the other side of it, the effect is no greater than might be expected from one pair of plates.

Volta, considering the changes consequent to his contrivance as the effect of a movement in the electric fluid, called the process electro-motion, and the plates producing it electromotors. But the phenomena show that the plates, as I have arranged them, are calori-motors, or heat movers, and the effect calori-motion. That this is a new view of the subject, may be inferred from the following passage in Davy's Elements. That great chemist observes, 'When very small conducting surfaces are used for conveying very large quantities of electricity, they become ignited; and of the different conductors that have been compared, charcoal is most easily heated by electrical discharges, next iron, platina, gold, then copper, and lastly, zinc. The phenomena of electrical ignition, whether taking place in gaseous, fluid, or solid bodies, always seem to be the results of a violent exertion of the electrical attractive and repellent powers, which may be connected with motions of the particles of the substances affected. That no subtile fluid, such as the matter of heat has been imagined to be, can be discharged from

these substances, in consequence of the effect of the electricity, seems probable, from the circumstances, that a wire of platina may be preserved in a state of intense ignition in vacuo, by means of the Voltaic apparatus, for an unlimited time; and such a wire cannot be supposed to contain an inexhaustible quantity of subtile matter.'

But I demand where are the repellent and attractive powers to which the ignition produced by the *Calorimotor* can be attributed? Besides, I would beg leave respectfully to inquire of this illustrious author, whence the necessity of considering the heat evolved under the circumstances alluded to as the effect of the electrical fluid; or why we may not as well suppose the latter to be excited by the heat? It is evident, as he observes, that a wire cannot be supposed to contain an inexhaustible supply of matter however subtile; but wherefore may not one kind of subtile matter be supplied to it from the apparatus as well as another; especially, when to suppose such a supply is quite as inconsistent with the characteristics of pure electricity, as with those of pure caloric? . . .

For the purpose of ascertaining the necessity of the alternation and proximity of the copper and zinc plates, it has been mentioned that distinct square sheets were employed. The experiments have since been repeated and found to succeed by Dr. Patterson and Mr. Lukens, by means of two continuous sheets, one of zinc, the other of copper, wound into two *concentric coils* or *spirals*. This, though the circumstance was not known to them, was the form I had myself proposed to adopt, and had suggested as convenient for a Galvanic apparatus to several friends at the beginning of the winter; though the consideration above stated induced me to prefer for a first experiment a more manageable arrangement.

Since writing the above, I find that when, in the apparatus of twenty copper and twenty zinc plates, ten copper plates



on one side are connected with ten zinc on the other, and a communication made between the remaining twenty by a piece of iron wire, about the eighth of an inch in diameter, the wire enters into a vivid state of combustion on the immersion of the plates. Platina wire equal to No. 18 (the largest I had at hand) is rapidly fused if substituted for the iron.

This arrangement is equivalent to a battery of two large Galvanic pairs; excepting that there is no insulation, all the plates being plunged in one vessel. I have usually separated the pairs by a board, extending across the frame merely.

Indeed, when the forty plates were successively associated in pairs, of copper and zinc, though suspended in a fluid held in a common recipient without partitions; there was considerable intensity of Galvanic action. This shows that, independently of any power of conducting electricity, there is some movement in the solvent fluid which tends to carry forward the Galvanic principle from the copper to the zinc end of the series. I infer that electro-caloric is communicated in this case by circulation, and that in non-elastic fluids the same difficulty exists as to its retrocession from the positive to the negative end of the series, as is found in the downward passage of caloric through them.

It ought to be mentioned, that the connecting wire should be placed between the heterogeneous surfaces before their immersion, as the most intense ignition takes place immediately afterward. If the connexion be made after the plates are immersed, the effect is much less powerful; and sometimes after two or three immersions the apparatus loses its power, though the action of the solvent should become in the interim much more violent. Without any change in the latter, after the plates have been for some time suspended in the air, they regain their efficacy. I had observed in a Galvanic pile of three hundred pairs of two inches square, a like consequence resulting from a simultaneous immersion of

the whole. The bars holding the plates were balanced by weights, as window sashes are, so that all the plates could be very quickly dipped. A platina wire, No. 18, was fused into a globule, while the evolution of potassium was demonstrated by a rose-coloured flame arising from some potash which had been placed between the poles. The heat however diminished in a few seconds, though the greater extrication of hydrogen from the plates indicated a more intense chemical action.

Agreeably to an observation of Dr. Patterson, electrical excitement may be detected in the apparatus by the condensing electroscope; but this is no more than what Volta observed to be the consequence of the contact of heterogeneous metals.

The thinnest piece of charcoal intercepts the calorific agent, whatever it may be. In order to ascertain this, the inside of a hollow brass cylinder, having the internal diameter two inches, and the outside of another smaller cylinder of the same substance, were made conical and correspondent, so that the greater would contain the less, and leave an interstice of about one-sixteenth of an inch between them. This interstice was filled with wood, by plugging the larger cylinder with this material, and excavating the plug till it would permit the smaller brass cylinder to be driven in. The excavation and the fitting of the cylinders was performed accurately by means of a turning lathe. The wood in the interstice was then charred by exposing the whole covered by sand in a crucible to a red heat. The charcoal, notwithstanding the shrinkage consequent to the fire, was brought into complete contact with the inclosing metallic surfaces by pressing the interior cylinder further into the exterior one.

Thus prepared, the interior cylinder being made to touch one of the Galvanic surfaces, a wire brought from the other Galvanic surface into contact with the outside cylinder, was not affected in the least, though the slightest touch of the interior one caused ignition. The contact of the charcoal with



the containing metals probably took place throughout a superficies of four square inches, and the wire was not much more than the hundredth part of an inch thick, so that unless it were to conduct electrical about forty thousand times better than the charcoal, it ought to have been heated; if the calorific influence of this apparatus result from electrical excitement.

I am led finally to suppose, that the contact of dissimilar metals, when subjected to the action of solvents, causes a movement in caloric as well as in the electric fluid, and that the phenomena of Galvanism, the unlimited evolution of heat by friction, the extrication of gaseous matter without the production of cold, might be all explained by supposing a combination between the fluids of heat and electricity. We find scarcely any two kinds of ponderable matter which do not exercise more or less affinity towards each other. Moreover, imponderable particles are supposed highly attractive of ponderable ones. Why then should we not infer the existence of similar affinities between imponderable particles reciprocally? That a peculiar combination between heat and light exists in the solar beams, is evident from their not imparting warmth to a lens through which they may pass, as do those of our culinary fires.

Under this view of the case, the action of the poles in Galvanic decomposition is one of complete affinity. The particles of compounds are attracted to the different wires agreeably to their susceptibilities to the positive and negative attraction, and the caloric, leaving the electric fluid with which it had been combined, unites with them at the moment that their electric state is neutralized.

As an exciting fluid, I have usually employed a solution of one part sulphuric acid, and two parts muriate of soda with seventy of water; but, to my surprise, I have produced nearly a white heat by an *alkaline solution* barely sensible to the taste.

For the display of the heat effects, the addition of manganese, red lead, or the nitrates, is advantageous.

The rationale is obvious. The oxygen of these substances prevents the liberation of the gaseous hydrogen, which would carry off the caloric. Adding to diluted muriatic acid, while acting on zinc, enough red lead to prevent effervescence, the temperature rose from 70 to 110 Fahrenheit.

The power of the *calorimotor* is much increased by having the communication between the different sheets formed by very large strips or masses of metal. Observing this, I rendered the sheets of copper shorter by half an inch, for a distance of four inches of their edges, where the communication was to be made between the zinc sheets; and, vice versa, the zinc was made in the same way shorter than the copper sheets where these were to communicate with each other. The edges of the shortened sheets being defended by strips of wood, tin was cast on the intermediate protruding edges of the longer ones, so as to embrace a portion of each equal to about one quarter of an inch by four inches. On one side, the tin was made to run completely across, connecting at the same time ten copper and ten zinc sheets. On the other side there was an interstice of above a quarter of an inch left between the stratum of tin embracing the copper, and that embracing the zinc plates. On each of the approaching terminations of the connecting tin strata was soldered a kind of forceps, formed of a bent piece of sheet brass, furnished with a screw for pressing the jaws together. The distance between the different forceps was about two inches. The advantage of a very close contact was made very evident by the action of the screws; the relaxation or increase of pressure on the connecting wire by turning them being productive of a correspondent change in the intensity of ignition.

It now remains to state, that by means of iron ignited in this apparatus, *a fixed alkali may be decomposed extem-*



*poraneously*. If a connecting iron wire, while in combustion, be touched by the hydrate of potash, the evolution of potassium is demonstrated by a rose-coloured flame. The alkali may be applied to the wire in small pieces in a flat hook of sheet iron. But the best mode of application is by means of a tray made by doubling a slip of sheet iron at the ends, and leaving a receptacle in the centre, in which the potash may be placed covered with filings. This tray being substituted for the connecting wire, as soon as the immersion of the apparatus causes the metal to burn, the rose-coloured flame appears, and if the residuum left in the sheet iron be afterward thrown into water, an effervescence sometimes ensues.

I have ascertained that an iron heated to combustion, by a blacksmith's forge fire, will cause the decomposition of the hydrate of potash.

The dimensions of the *Calorimotor* may be much reduced without proportionately diminishing the effect. I have one of sixty plates within a cubic foot, which burns off No. 16, iron wire. A good workman could get 120 plates of a foot square within a hollow cube of a size no larger. But the inflammation of the hydrogen which gives so much splendour to the experiment, can only be exhibited advantageously on a large scale."

There we have the outline of an early instrument which was to prove to be more helpful in his subsequent work, and it was indeed most suggestive. In fact "in Hare's calorimotor we have a form of apparatus which is admirably adapted to develop a large quantitative flow, and one which has now a wide use for this purpose, the substitution of plates of carbon for copper and of amalgamated zinc for the unprotected metal, being the only changes which modern art has introduced into Hare's original instrument, long forgotten, and perhaps before unknown to the present generation, but now revived again, and permanently installed in the laboratory of the physicist."

Silliman said of the calorimotor: "its principal obvious effect is to produce a great flow of heat with very little electrical excitement; in this view it is a peculiar and interesting instrument, and the name given by the inventor is entirely appropriate; he might also with almost equal propriety have called it a magnetimotor."

And further: "in the calorimotor in my possession the plates are 18 inches square; there are 9 of zinc on one side, alternating with 10 of copper, and 10 of zinc on the other side alternating with 11 of copper, 40 plates in the whole, and 90 square feet of surface; the outside plate is copper on both sides, so that the zinc surfaces are, everywhere, opposed to copper, and this is all the insulation that there is, as the cubical box into which they are plunged has no partition. The plates are connected by long bars of tin, gashed by a saw, so as to receive the metals, which are secured also by solder, and the alternating plates are cut down, so as not to be in the way of the bars that connect the opposing surfaces. *Not only alternation of the plates but a repetition of the pairs, to at least two, is necessary to produce an intense calorific effect.*"

Alfred Niaudet, in his *Traité Élémentaire de la Pile Électrique*, said of the Calorimotor that it was worthy of especial consideration, because it had served as a model for Planté in the construction of his secondary battery.

It might be argued that matters so significant as those just described would have crowded out all other ideas, but in the midst of this epoch-making work one may read in the *Portfolio* (1818) that Hare had contrived an apparatus for the burning of tar instead of oil, to be applied in the lighting of cities, manufactories, etc., greatly diminishing the expense. He had ascertained that three pounds of tar would give as much light as two pounds of oil or tallow burnt in the usual manner, and, consequently, calculating on the usual prices of these articles, and the entire saving for wicks, which were



not required for the burning of tar, it appeared that the same quantity of light might be produced in this way at a very much reduced cost. The apparatus consisted of a fountain reservoir to hold four or five pounds of tar to supply the lamp at a uniform height, "and a lanthorn with a draught pipe attached to it." The lamp presented at one end a cylindrical mouth for receiving the pipe of the reservoir; at the other end a cylindrical cup, in which the tar was ignited, the flame being drawn up through a central hole in the bottom of the lanthorn so as to occupy its axis in passing to the draught pipe. All the air which supplied this was made to meet in the same axis, and thus to excite the combustion. A lamp of this description would burn for nine hours, and it was found that by it the carbonaceous matter, which usually obscures the flame of resinous substances, was made to contribute to the light. Four or five barrels of tar used in this way, and they did not cost more than ten to twelve dollars, it was computed would give eight times the light of a common street lamp for one year.

Absorbed as Hare must have been in the novel and far-reaching effects of his calorimotor, he nevertheless took occasion to remind Silliman that he was teaching "that acid properties never appearing in the absence of water, this fluid or its elements are most entitled to be considered as the acidifying principle; but that probably it does not exist in acids as water, but is decomposed when added to them, the particles of hydrogen and oxygen by their different polarities taking opposite sides of those composing the base. The extrication of hydrogen by the action of diluted sulphuric acid on iron or zinc, being the consequence of a previous not simultaneous decomposition of water. Hence when sulphuric or nitric acids are so concentrated as to char or ignite, they are not acids really."

And in a letter to the same person, dated December 30, 1819, he observed:

“I believe I mentioned in a letter to you last summer, that I had rendered the flame of Hydrogen luminous like that of oil, by adding a small quantity of oil of turpentine to the usual mixture for generating that gas. When the ingredients are at the proper temperature the light is greater I think than that produced by Carburetted Hydrogen.

I have lately found that the addition of about 1/17 of the same substance to alcohol will give this fluid the property of burning with a highly luminous flame, and that there is a certain point in the proportions at which the mixture burns without soot, like a gas light.

This observation may be of use where spirits are cheap, as in our western states, and even in the northern parts of the Union where it is made from potatoes.

It might be serviceable to morals if the value of this article could be enhanced by a *new* mode of consumption.”

The account of the decomposition of caustic potash *extemporaneously* was issued in a separate pamphlet. This was sent to Silliman who probably questioned the word *extemporaneously*, which then called forth this letter:

“My dear Silliman

In answer to yours of the 11<sup>th</sup> you may alter the title agreeably to your judgment which I have not the least doubt is better in this case than mine can be but have you reflected on the word *extemporaneously*. I do not say a new mode of decomposing potash but a new mode of decomposing it *extemporaneously*—It has not been effected heretofore in any mode so *extemporaneous*—especially by iron—Then I do not say a new mode of obtaining potassium. The decomposition of the potash which is a different thing is proved by the Flame which has the rose colour arising from the metal or the Potassuretted Hydrogen—The paper was ordered to be printed in the journal of the society but so much delay was likely to take place & having a prospect of being obliged to



leave Philad<sup>a</sup> I put it into the hands of Mess<sup>r</sup> Carey & Son who published it giving me a number of copies.—You may say in a note this paper was read before the Academy by D<sup>r</sup> Hare & was ordered to be printed in their journal but more delay occurring than usual the author prefer'd publishing it himself—

You are at liberty of course to republish it merely copying the title if you should not deem any explanation necessary—You can have the plate that is purchase it at \$12 & I have accordingly made the bargain & will send it on—You might have the Calorimotor constructed here for about ten for the labour I presume—The workman who made mine charges about 1 33/100 p day. The whole cost fifty dolls probably on a large scale—

Your faithful fr<sup>d</sup>

ROB<sup>t</sup> HARE."

"I am grieved that your sons ill health should afflict you so much—We know by experience what must be your sufferings."

It was in 1818 that Silliman, feeling the need of a depository for the discoveries of American men of Science, founded the *American Journal of Science*. It had a rather chequered career in its earlier years, as is apparent from this letter from Hare:

" . . . I am grieved to hear the pecuniary result of your publication is so unfavorable. In our city the interest in favor of our own journals is very strong. I have already hinted this to you as operating against the giving of communications abroad, and of course it will operate against subscriptions. There are few in our country who take interest in the profounder branches of knowledge. I doubt if there be a dozen men on the Continent who would peruse some of the essays on musical temperament in your Journal.

I was told in New York that many said they could not understand my memoir, who considered their standing such as to feel as if this were an imputation against me rather than themselves. I could not write it for those who are so ignorant, without making it too prolix and commonplace for adepts. There is our difficulty,—we cannot write anything for the scientific few which will be agreeable to the ignorant many . . .”

“ My dear Silliman “ Philad<sup>a</sup> April 30<sup>th</sup> 1820

Mess<sup>rs</sup> Little & Henry have informed me that another number of your journal is already published. I was led to infer from your letter last winter that there would not be room for my strictures on Clarke’s gas blowpipe as it will occupy nearly two sheets of printing on the scale of your work. I am sensible it is difficult to get people to read so much on a subject which does not interest them but I have been desirous of giving a full exposure of the flagitious conduct of that shallow pretender.—I have a number of subjects to write on which I intend to embody in the appendix to the text-book. Some of these being more brief & interesting especially my improvements in Eudiometry it seems to me that my publication would command more attention were the whole associated. The good will & opinion of my readers being gain’d by one topick they may be more dispos’d to give a fair attention to the other. I contemplate therefore publishing a pamphlet comprising my strictures & other matter together publish’d as an appendix to the text Book used in the University of Pennsylvania—The expense of the engravings will be borne eventually by M<sup>r</sup> DeSilver the publisher of the text Book. It remains for you to say whether you will wish the use of the plates & if so whether they shall be executed for you or him in the first instance. If desirable & practicable I would be willing to have the whole printed as if originally for your journal—But I fear



this will not be possible as I am desirous that my reply to Clarke should appear this spring in time to reach Europe in the spring vessels. So far as respects the circulation of your journal in this country the publication of these articles in the mode first suggested would have little influence as the circulation of the letter would be very limited & in that case you might select or abridge as might be agreeable to you while I should stipulate for the use of the plates on moderate terms.

You have never told me whether to send on your lottery ticket or to keep it for you—The number is Three hundred & seventy two (372)—

I believe I never mentioned to you that I spoke to Th<sup>o</sup> Duncan the workman who made my calorimotor to construct one for you but he did not offer to go to work till it was too late in the season to be in time according for your orders.

Can you not pay us a visit this vacation? It would give M<sup>rs</sup> Hare & myself great pleasure to see you & M<sup>rs</sup> Silliman under our roof—during your stay here?

Yours faithfully

ROB<sup>t</sup> HARE."

"P. S. Since writing the above I feel somewhat more undecided about the course which it would be preferable to pursue. I should be glad to hear from you."

To-day gas analysis has attained a high degree of perfection. It had scarcely been begun in the first quarter of the 19th Century. The inquiring mind of Hare, impelling him forward in every variety of research, encountered this situation so that he promptly strove to improve conditions and advance eudiometric studies. Indeed, a careful examination of the numerous forms of eudiometer devised by him leads to the evident conclusion that in this field he was also a pioneer and that in his varied apparatus one sees the germs of more modern and widely celebrated apparatus. With his eudiometer not only was air analyzed, but ammonia, the hy-

drides of carbon and other gaseous mixtures. The ignition of the platinum by the Calorimotor, for the purpose of inflaming the gases, is an elegant and novel method of operating; the various modes of measuring the gases are ingenious and accurate, but the detailed description of all the instruments and operations may be found in the celebrated "Compendium." Therefore, it will probably best serve the purpose of the reader to follow the exact language of the master in the account of his first instrument for this kind of work.

"Among the operations of chemistry, none probably are more difficult than those called Eudiometrical, in which aeriform substances are analyzed.

Elastic fluids are so liable to contract or expand with the slightest change of temperature or pressure, that it is requisite to have the surface of the portion under water or mercury employed to confine it, and the heat of the hand may render the result inaccurate. There is no simple mode of causing the surface of the gas in measure glass to form a plane corresponding with the brim of the measure glass containing it. The transfer of small portions of gas without loss, especially from large bells into small tubes is very difficult. Hence there is trouble, delay and waste.

I shall proceed to describe some instruments which I have lately invented, and which appear to be free from the disadvantages above described. They are all essentially dependent on one principle for their superiority. A recurved glass tube is furnished with a sliding wire of iron or copper, graduated into two hundred parts. The process of making wire by drawing it through a hole, renders its circumference of necessity everywhere equal and homologous. Consequently equal lengths will contain equal bulks.

The wire slides through a cork soaked in beeswax and oil, and compressed by a screw, so that neither air nor water can pass by it.



The length of the longer leg is fifteen inches, that of the shorter one six inches. The bore of the tube is from  $\frac{4}{10}$  to  $\frac{5}{10}$  an inch in diameter, but converges towards the termination of the shorter leg to an orifice about large enough to admit a brass pin. Over this a screw is sometimes affixed, so as to close it when necessary.

The tube being filled with water or mercury, and the wire pushed into it as far as it can go, on drawing this out again any desired distance, an equivalent bulk of air must enter the capillary orifice if open. By forcing the rod back again into the tube, the air must be proportionately excluded. Thus the movements of the sliding wire are accompanied by a corresponding ingress or egress of air, and to know how many divisions of the former have been pushed into the tube, or withdrawn from it, is the same as to know how much air has been drawn in or expelled.

If, instead of allowing the orifice to be in the open air, it be introduced within a bell glass, holding gas over the pneumatic apparatus, on pulling out the wire, there will be a corresponding entrance of gas into the instrument; and it must be evident that if the point of the gas measures be transferred to the interior of any other recipient, the gas which had entered, or any part of it, may be made to go into any such recipient by reversing the motion of the wire. As the hands are, during this operation, remote from the part of the tube which contains the aeriform matter, no expansion can arise from this source, and the operation is so much expedited, that there is much less chance of variation from any other cause. By taking care to have the surface of the gas in the bell glasses below that of the fluid in the cistern, the density of the former will be somewhat too great, but on bringing the orifice of the gas measurer on a level, with the surface of the fluid in the cistern, the gas, no longer subject to any extra pressure, will assume its proper volume, the

excess being seen to escape in bubbles. Should the tube in lieu of water, be filled with any solution, calculated to absorb any gas, of which the proportion, in any mixture, is to be ascertained, and if the quantity of absorption which can take place while the wire is drawing out, is deemed unworthy of attention, we have only to introduce the shorter leg of the tube into the containing vessel, as above described, and draw out the wire to two hundred on its scale, then depressing the point below the surface of the fluid in one pneumatic cistern in the usual time with due agitation, all the gas which the fluid can take up, will disappear. The quantity will be represented by the number of divisions which remain without the tube, after pushing in the wire just so far, as to exclude the residual gas.

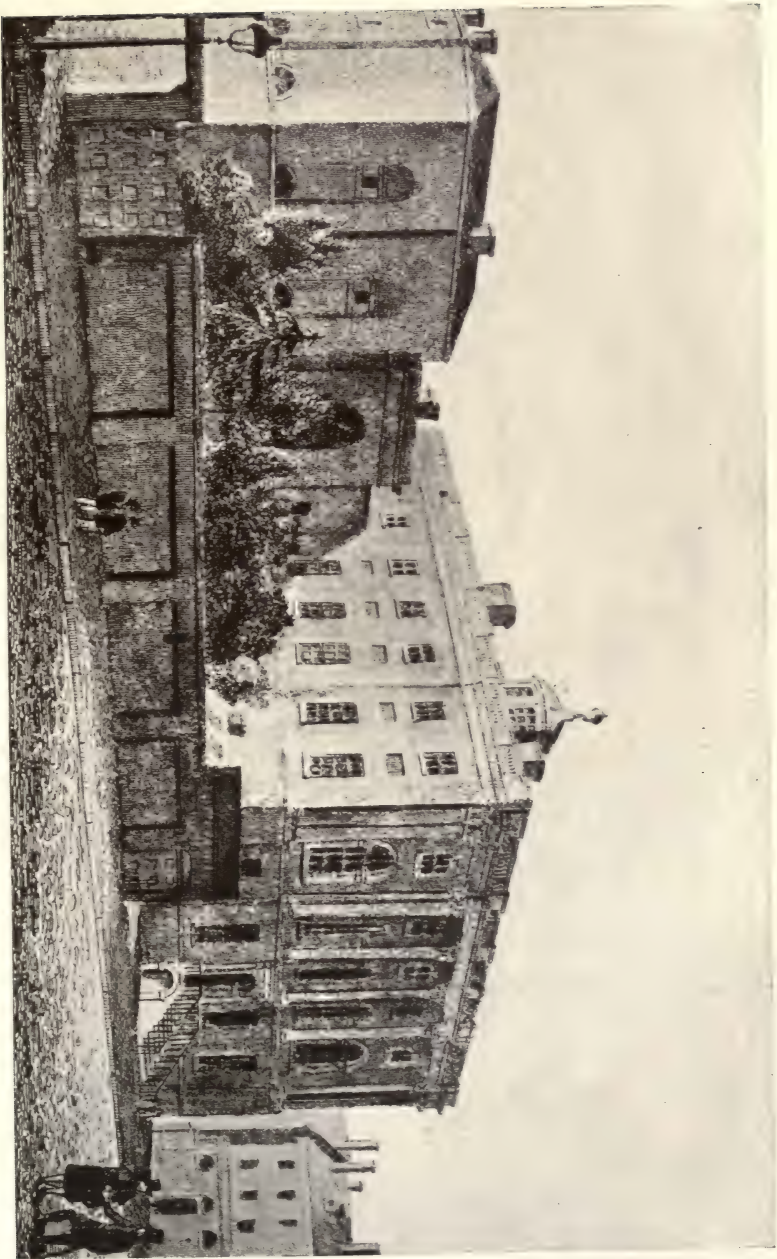
Should it be deemed an object to avoid the possibility of any absorption during the time occupied in the retraction of the sliding wire, or should it be desired to expose the gas to a larger quantity of the absorbing fluid, an additional vessel is used, which is of an oblate spheroidal form, with a large neck, ground to fit on the shorter leg of a gas measurer, and furnished at the opposite apex with a tube, of which the bore converges to a capillary opening, surmounted by a screw, as already described, on the point of the gas measurer simply. This vessel (in shape not unlike a turnip) is filled with the absorbing fluid, and the gas measurer being duly charged with gas as above described, inserted into it. By the action of the sliding wire, the gas is propelled into the spheroid, where, by agitation and time the absorption is completed. Meanwhile the orifice of the spheroid should be kept open, and under water, so as to permit the latter to take place of that portion of the gas which disappears.—Whatever remains unabsorbed, is expelled from the glass spheroid, as in the case of the tube when used alone; and the divisions on the rod remaining without, will shew how much the fluid has taken up.



When atmospheric air, or oxygen gas is to be analyzed by nitrous gas, the glass spheroid is filled with water, and inverted with its orifice closed over the well of the pneumatic cistern. It should be supported by a wire stand, so as to leave the neck unobstructed. Any number of measures of nitrous gas, and of oxygen gas or atmospheric air, may then be drawn into the measurer, and expelled into the spheroid successively, and the absorption estimated as already explained. When the residuum is too great to be expelled by returning the whole of the rod into the tube, by depressing the orifice of the spheroid just under the surface of water, the wire may be again gently retracted, water taking its place; and the movement may thus be alternated, till the whole of the remaining gas is excluded. In order to apply this principle to Volta's process of ascertaining by explosion the quantity of hydrogen or oxygen gas present, in a mixture, the gas measurer is made as much stronger, as eudiometers are usually, when intended to be so used. It is in like manner drilled so as to receive wires for passing the electric spark. The instrument being charged with the gases successively in any required proportion, closed by the screw, and an explosion accomplished; to fill any consequent vacuity, the orifice is to be opened just below the surface of water or mercury. The quantity destroyed by the combustion is then ascertained by the sliding wire.

This experiment is more accurately performed by means of mercury than water. From this fluid, concussion, or even the partial vacuum produced by the gaseous matter, may extricate air, and thus vitiate results. There ought always to be a considerable excess of gas not liable to be acted on. The activity of the inflammation is lessened, and the unconsumed air breaks the shock.

I have found the galvanic ignition produced by a small *calorimotor* preferable to the electric spark. Suppose a piece



THE SECOND HOME OF THE UNIVERSITY OF PENNSYLVANIA  
The wing on the left contained the Laboratory and Lecture Room of Robert Hare





of iron wire to be filed down in the middle for about one half of an inch to about one third of the original diameter. The whole is cemented into the perforation drilled in the tube, so as that the smallest part may extend across the bore. The wire should then be cut off at about one-third of an inch from the tube, so as to stand out from it on each side about that distance. If these protruding wires be severally placed in the forceps of a *calorimotor* and the plates subjected to an acid, the small part of the wire within the tube is vividly ignited, and any gas in contact with it must explode. The interior wire is best made of platina, and may in that case be screwed into two larger pieces of a baser metal; or a baser metal may be fastened on it, by drawing through a wire plate, and the platina duly denuded by a file where it crosses the bore.

The *calorimotor* which I have used for this purpose, consists of eleven plates of copper, and a like number of zinc, placed alternately within one-fourth of an inch of each other; those of the same kind of metal being all associated by means of a metallic stratum of tin cast over them. The two heterogeneous galvanic surfaces thus formed, have each soldered to them a wire in a vertical position, and slit, so as to present a fork or snake's mouth. The wires are just so far apart as to admit the gas measurer between them, so that the wires of the latter may easily be pressed into the snake mouths. It is better that the wires of the gas measurer should be flattened in such manner as to present a larger surface for contact. There must also be an oblong square box or hollow parallelopipedon of such a width as just to admit the *calorimotor*, and more than double its length and depth. The *calorimotor* is placed within this box, at one end of it, about an inch below the brim. Dilute acid is poured in so as to occupy the lower half of the vessel, until it nearly reaches the plates. A plunger, consisting of a water tight box, or solid block of wood, is then made to occupy the other side of the little cistern.



The depression of this causes the rise of the acid among the plates in the *calorimotor*, and consequently the ignition of a wire forming a communication between the surfaces.

This apparatus may be constructed in the circular form, by so placing two concentric coils, or several concentric hollow cylinders of copper and zinc, alternately within the upper half of a glass jar as to admit of a plunger in the middle, which in this case may be of an apothecaries stopper round or bottle. The acid solution must occupy the lower half of the vessel, unless when the plunger raises it.

I am under the impression that there is no form in which a pair of galvanic surfaces can be made so powerful in proportion to their extent, as in that above mentioned. The zinc is everywhere opposed by two copper surfaces by having this metal only a small fraction in excess."

The story of the development of the *deflagrator*—a second epoch-making instrument—is best told in Hare's own language:

"I had observed that the ignition produced by one or two galvanic pairs attained its highest intensity, almost as soon as they were covered by the acid used to excite them, and ceased soon afterwards; although the action of the acid should have increased during the interim. I had also remarked in using an apparatus of three hundred pairs of small plates, that a platina wire, No. 16, placed in the circuit, was fused in consequence of a construction which enabled me to plunge them all nearly at the same time. It was therefore conceived, that the maximum of effect in voltaic apparatus of extensive series had never been attained. The plates are generally arranged in distinct troughs rarely containing more than twenty pairs. Those of the great apparatus of the Royal Institution, employed by Sir H. Davy, had only ten pairs in each. There were one hundred such to be successfully placed

in the acid, and the whole connected ere the poles could act. Consequently the effect which arises immediately after immersion, would be lost in the troughs first arranged, before it could be produced in the last; and no effort appears to have been made to take advantage of this transient accumulation of power, either in using the magnificent combination, or in any other of which I have read. In order to observe the consequence of simultaneous immersion with a series sufficiently numerous to test the correctness of my expectations, a galvanic apparatus of eighty concentric coils of copper and zinc was so suspended by a beam and levers, as that they might be made to descend into, or rise out of the acid in an instant. The zinc sheets were about nine inches by six, the copper fourteen by six; more of this metal being necessary, as in every coil it was made to commence within the zinc, and completely to surround it without. The sheets were coiled so as not to leave between them an interstice wider than a quarter of an inch. Each coil is in diameter about two inches and a half, so that all may descend freely into eighty glass jars two inches and three quarters diameter inside, and eight inches high, duly stationed to receive them.

My apparatus being thus arranged, two small lead pipes were severally soldered to each pole, and a piece of charcoal about a quarter of an inch thick, and an inch and a half long, tapering a little at each extremity, had these severally inserted into the hollow ends of the pipes: The jars being furnished with diluted acid and the coils suddenly lowered into them, no vestige of the charcoal could be seen: It was ignited so intensely, that those portions of the pipes by which it had been embraced were destroyed. In order to avoid a useless and tiresome repetition, I will here state that the coils were only kept in the acid while the action at the poles was at a maximum in the experiment just mentioned, and in others which I am about to describe, unless where the decomposition



produced by water is spoken of, or the sensation excited in the hands. I designate the apparatus with which I performed them, as the galvanic *deflagrator*, on account of its superior power, in proportion to its size, in causing deflagration; and as, in the form last adopted, it differs from the voltaic pile in the omission of one of the elements heretofore deemed necessary to its construction.

Desirous of seeing the effect of the simultaneous immersion of my series upon water, the pipes soldered to the poles were introduced into a vessel containing that fluid. No extraordinary effect was perceived, until they were very near, when a vivid flash was observed, and happening to touch almost at the same time, they were found fused and incorporated at the place of contact. I next soldered to each pipe a brass cylinder about five-tenths of an inch bore. These cylinders were made to receive the tapering extremities of a piece of charcoal about two inches long so as to complete the circuit. The submersion of the coils caused the most vivid ignition in the coal. It was instantaneously and entirely on fire. A piece of platina of about a quarter of an inch diameter in connexion with one pole, was instantly fused at the end on being brought in contact with some mercury communicating with the other. When two cylinders of charcoal having hemispherical termination were fitted into the brass cylinders and brought nearly into contact, a most *vivid ignition* took place, and continued after they were removed about a half or three-quarters of an inch apart, the *interval rivalling the sun in brilliancy*. The igneous fluid appeared to proceed from the positive side. The charcoal in the cylinder soldered to the latter would be intensely ignited throughout when the piece connected with the negative pole was ignited more towards the extremity approaching the positive. The most intense action seems to arise from placing a platina wire of about the eighth of an inch diameter, in connexion with the

positive pole, and bringing it in contact with, and afterwards removing it a small distance apart from, a piece of charcoal (fresh from the fire) affixed to the other pole.

As points are pre-eminently capable of carrying off (without being injured) a current of the electrical fluid, and very ill qualified to conduct caloric; while by facilitating radiation, charcoal favours the separation of caloric from the electricity which does not radiate; this result seems consistent with my hypothesis, that the fluids as extricated by Volta's pile is a *compound of caloric and electricity*; but not with the other hypothesis, which supposes it to be *electricity alone*. The finest needle is competent to discharge the product of the most powerful machines without detriment, if received gradually as generated by them. Platina points, as small as those which were melted like wax in my experiments, are used as tips to lightning rods without injury, unless in sudden discharges, produced under peculiar circumstances.

The following experiment I conceive to be very unfavourable to the idea that *galvanic ignition* arises from a *current of electricity*.

A cylinder of lead about a quarter of an inch diameter, and about two inches long, was reduced to the thickness of a common brass pin for about three quarters of an inch. When one end was connected with one pole of the apparatus, the other remained suspended by this filament; yet it was instantaneously fused by contact with the other pole. As all the calorific fluid which acted upon the suspended knob, must have passed through the filament by which it hung, the fusion could not have resulted from a pure electrical current, which would have dispersed the filament ere a mass fifty times larger had been perceptibly affected. According to my theory, caloric is not separated from the electricity until circumstances very much favour a disunion, as on the passage of the compound fluid through charcoal, the air, or a



vacuum. In operating with the *deflagrator*, I have found a brass knob of about five tenths of an inch in diameter, to burn on the superficies only; where alone according to my view, caloric is separated so as to act on the mass. Having, as mentioned in the memoir on my theory of galvanism, found that four galvanic surfaces acted well in one recipient, I was tempted by means of the eighty coils to extend that construction. It occurred to me that attempts of this kind, had failed from using only one copper for each zinc plate. The zinc had always been permitted to react towards the negative, as well as the positive pole. My coils being surrounded by copper, it seemed probable, that, if electro-caloric were, as I had suggested, carried forward by circulation arising from galvanic polarity, this might act within the interior of the coils, yet not be exerted between one coil and another.

I had accordingly a trough constructed with a partition along the middle, so as to receive forty coils on one side, and a like number on the other. This apparatus when in operation excited a sensation scarcely tolerable in the backs of the hands. Interposed charcoal was not ignited as easily as before, but a most intense ignition took place on bringing a metallic point connected with one pole of the series, into contact with a piece of charcoal fastened to the other. It did not take place, however, so speedily as when glasses were used; but soon after the ignition was effected it became even more powerful than before. A cylinder of platina nearly a quarter of an inch in diameter, tapering a little at the end, was fused and burned so as to sparkle to a considerable distance around, and fall in drops. A ball of brass of about half an inch diameter was seen to burn on its surface with a green flame. Tin foil, or tinsel rolled up into large coils of about three quarters of an inch thick, were rapidly destroyed, as was a wire of platina of No. 16. Platina wires in connexion with the poles were brought into contact with

sulphuric acid; there was an appearance of lively ignition, but strongest on the positive side. Excepting in its power of permeating charcoal, the galvanic fluid seemed to be extricated with as much force, as when each coil was in a distinct glass. Apprehending that the partition in the trough did not sufficiently insulate the poles from each other, as they were but a few inches apart, moisture or moistened wood intervening, I had two troughs each to hold forty pairs, and took care that there should be a dry space about four inches broad between them. They were first filled with pure river water, there being no saline nor acid matter to influence the plates, unless the very minute quantity which might have remained on them from former immersions. Yet the sensation produced by them, on the backs of my hands, was painful; and a lively scintillation took place when the poles were approximated. Dutch gold leaf was not sensibly burned, though water was found decomposable by wires properly affixed. No effect was produced on potash, the heat being inadequate to fuse it.

A mixture of nitre and sulphuric acid was next added to the water in the troughs, afterwards charcoal from the fire was vividly ignited, and when attached to the positive pole, a steel wire was interposed between it and the other pole, the most vivid ignition which I ever saw was induced. I should deem it imprudent to repeat the experiment without glasses, as my eyes, though unusually strong, were affected for forty-eight hours afterwards. If the intensity of the light did not produce an optical deception, but its distressing influence upon the organs of vision, the charcoal assumed a pasty consistence, as if in a state approaching to fusion. That charcoal should be thus softened, without being destroyed by the oxygen of the atmosphere, will not appear strange, when the power of galvanism in reversing chemical affinities is remembered; and were it otherwise, the air could have no access, first, because of the excessive rarefaction, and in the next



place as I suspect on account of the volatilization of the carbon forming about it a circumambient atmosphere. This last mentioned impression arose from observing, that when the experiment was performed in vacuo, there was a lively scintillation, as if the carbon in an aeriform state acted as a supporter of combustion on the metal.

A wire of platina No. 16, was fused into a globule on being connected with the positive pole, and brought into contact with a piece of pure hydrate of potash, situated on a silver tray in connexion with the other pole. The potash became red hot, and was deflagrated rapidly with a flame having the rosy hue of potassuretted hydrogen.

The great apparatus of the Royal Institution, *in projectile power* was from six to eight times more potent than mine. It produced a discharge between charcoal points when removed about four inches apart, where mine will not produce a jet at more than three-fourths of an inch. But that series was two thousand, mine only about a twenty-fifth part as large.

A steel wire of about one tenth of an inch in diameter, affixed to the negative pole, was passed up through the axis of an open decked inverted bell glass, filled with water. A platina wire, No. 16, attached to a positive pole being passed down to the steel wire, both were fused together, and cooling, could not be separated by manual force. Immediately after this incorporation of their extremities, the platina wire became incandescent for a space of some inches above the surface of the water.

A piece of silvered paper about two inches square was folded up, the metallic surface outward, and fastened into vices affixed to the poles. Into each vice a wire was screwed at the same time. The fluid generated by the apparatus was not perceptibly conveyed by the silvered paper, as it did not prevent the wires severally attached to the poles from decomposing water or producing ignition by contact.

In my memoir on my theory of galvanism I suggested, that the decomposition of water, which Wollaston effected by mechanical electricity, might not be the effect of a divellent attraction like those excited by the poles of a voltaic pile, but of a mechanical concussion, as when wires are dispersed by the discharge of an electrical battery. In support of that opinion I will now observe, that he could not prevent hydrogen and oxygen from being extricated at each wire, instead of hydrogen being given off only at one, and oxygen at the other, as is invariably the case when the voltaic pile is employed. That learned and ingenious philosopher, in concluding his account of this celebrated experiment, says 'but in fact the resemblance is not complete, for in every way in which I have tried it, I observed each wire gave out both oxygen and hydrogen gas, instead of their being formed separately as by the electric pile.'

Is it not reasonable to suppose that an electrical shock may dissipate any body into its elementary atoms, whether simple or compound, so that no two particles would be left together which can be separated by physical means?

Looking over Singer's *Electricity*, a recent and most able modern publication, I find that in the explosion of brass wire by an electrical battery, the copper and zinc actually separated. He says, page 186, 'Brass wire is sometimes *decomposed* by the charge; the copper and zinc of which it is formed being separated from each other, and appearing in their distinct metallic colours.' On the next page in the same work, I find that the oxides of mercury and tin are reduced by electrical discharges. 'Introduce,' says the author, 'some oxide of tin into a glass tube, so that when the tube is laid horizontal, the oxide may cover about half an inch of its lower internal surface. Place the tube on the table of the universal discharger, and introduce the pointed wires into its opposite ends, that the portion of oxide may lie between them. Pass



several strong charges in succession through the tube, replacing the oxide in its situation, should it be *dispersed*. If the charges are sufficiently powerful, a part of the tube will soon be stained with metallic tin, which has been revived by the action of transmitted electricity.' It cannot be alleged that in such decompositions the divellent polar attractions are exercised like those which characterize the action of wire proceeding from the poles of a voltaic apparatus. The particles were dispersed from, instead of being attracted to the wires, by which the influence was conveyed among them. This being undeniable, it can hardly be advanced that we are to have one mode of explaining the separation of the elements of brass by an electrical discharge, another of explaining the separation of the elements of water by the same agent. One rationale when oxygen is liberated from tin, and another when liberated by like means from hydrogen. In the experiment in which copper was precipitated by the same philosopher at the negative pole, we are not informed whether the oxygen and acid in union with it were attracted to the other; and the changes produced in litmus are mentioned not as simultaneous, but successive. The violet and red rays of the spectrum have an opposite chemical influence in some degree like that of voltaic poles, but this has not led to the conclusion that the cause of galvanism and light is the same. Besides, admitting that the feeble results obtained by Wollaston and Van Marum are perfectly analogous to those obtained by the galvanic fluid, ere it can become an objection to my hypothesis, it ought first to be shown that the union between caloric and electricity, which I suppose productive of galvanic phenomena, cannot be produced by that very process. If they combine to form the galvanic fluid when extricated by ordinary galvanic action, they must have an affinity for each other. As I have suggested in my memoir, when electricity enters the pores of a metal it may unite with

its caloric. In Wollaston's experiments, being constrained to enter the metal, it may combine with enough of its caloric to produce, when emitted, results slightly approaching to those of a fluid in which caloric exists in greater proportion.

But once more I demand why, if mechanical electricity be too intense to produce galvanic phenomena, should it be rendered more capable of producing them by being still more concentrated.

If the one be generated more copiously, the other more intensely, the first will move in a large stream slowly, the last in a small stream rapidly. Yet by narrowing the channel of the latter, Wollaston is supposed to render it more like the former, that is, produces a resemblance by increasing the supported source of dissimilarity.

It has been imagined that the beneficial effect of his contrivance arises from the production of a continued stream, instead of a succession of sparks, but if a continued stream were the only desideratum, a point placed near the conductor of a powerful machine would afford this requisite, as the whole product may in such cases be conveyed by a sewing needle in a stream perfectly continuous. As yet no adequate reasons have been given why, in operating with the pile, it is not necessary, as in the processes of Van Marum and Wollaston, to enclose the wires in glass or sealing wax, in order to make the electricity emanate from a point within a conducting fluid. The absence of necessity is accounted for, according to my hypothesis, by the indisposition which the electric fluid has to quit the caloric in union with it, and the almost absolute incapacity which caloric has to pass through fluids unless by circulation. I conceive that in galvanic combinations, electro-caloric may circulate through the fluid from the positive to the negative surface, and through the metal from the negative to the positive. In the one case caloric subdues the disposition which electricity has to diffuse



itself through fluids, and carries it into circulation. In the other, as metals are excellent conductors of caloric, the prodigious power which electricity has to pervade them agreeably to any attractions which it may exercise, operates almost without restraining. This is fully exemplified in my galvanic deflagrator, where eighty pairs are suspended in two recipients, forty successively in each, and yet decompose potash with the utmost rapidity, and produce an almost intolerable sensation when excited only by fresh river water. I have already observed that the reason why galvanic apparatus composed of pairs consisting each of one copper and one zinc plate have not acted well without insulation, was because electro-caloric could retrocede in the negative, as well as advance in the positive direction. I will now add, that independently of the greater effect produced by the simultaneous immersion of my eighty coils, their power is improved by the proximity of the surfaces, which are only about an eighth of an inch asunder; so that the circulation may go on more rapidly.

Pursuant to the doctrine, which supposes the same quantity of electricity, varying in intensity in the ratio of the number of pairs to the quantity of surface, to be the sole agent in galvanic ignition, the electrical fluid as evolved by Sir H. Davy's great pile, must have been nearly two thousand times more intense, than as evolved by a single pair, yet it gives sparks at no greater distance than the thirtieth or fortieth of an inch. The intensity of the fluid must be at least as much greater in one instance, than in another, as the sparks produced by it are longer. A fine electrical plate machine of thirty two inches diameter, will give sparks at ten inches. Of course the intensity of the fluid which it emits, must be three hundred times greater than that emitted by two thousand pairs. The intensity produced by a single pair, must be two thousand times less than that produced by

a great pile, and of course six hundred thousand times less than that produced by a good electrical plate of thirty two inches. Yet a single pair of about a square foot in area, will certainly deflagrate more wire, than a like extent of coated surface charged by such a plate. According to Singer, it requires about one hundred and sixty square inches of coated glass, to destroy watch pendulum wire; a larger wire may be burned off by a galvanic battery of a foot square. But agreeably to the hypothesis in dispute, it compensates by quantity, for the want of intensity. Hence the quantity of fluid in the pair is six hundred thousand times greater, while its intensity is six hundred thousand times less; and vice versa of the coated surface. Is not this absurd? What does intensity mean as applied to a fluid? Is it not expressed by the ratio of quantity, to space? If there be twice as much electricity within one cubic inch, as within another, is there not twice the intensity? But the one acts suddenly, it may be said; the other slowly. But whence this difference? They may both have exactly the same surface to exist in. The same zinc and copper plates may be used for coatings first, and a galvanic pair afterwards. Let it be said, as it may in truth, that the charge is, in the one case attached to the glass superficies, in the other exists in the pores of the metal. But why does it avoid these pores in one case and reside in them in the other? What else resides in the pores of the metal which may be forced out by percussion? Is it not caloric? Possibly, unless under constraint, or circumstances favorable to a union between this principle and electricity, the latter cannot enter the metallic pores, beyond a certain degree of saturation; and hence an electrical charge does not reside in the metallic coatings of a Leyden phial, though it fuses the wire which forms a circuit between them.

It is admitted that the action of the galvanic fluid, is upon or between atoms; while mechanical electricity when



uncoerced, acts only upon masses. This difference has not been explained unless by my hypothesis, in which caloric, of which the influence is only exerted between atoms, is supposed to be a principal agent in galvanism. Nor has any other reason been given that water, which dissipates pure electricity, should cause the galvanic fluid to accumulate. From the prodigious effect which moist air, or a moist surface, has in paralyzing the most efficient machines, I am led to suppose, that the conducting power of moisture so situated, is greater than that of water under its surface. The power of this fluid to conduct mechanical electricity, is unfairly contrasted with that of a metal, when the former is enclosed in a glass tube, the latter bare.

According to Singer, the electrical accumulation is as great when water is used, as when more powerful menstrua are employed; but the power of ignition is wanting, until these are resorted to. De Luc showed, by his ingenious dissections of the pile, that electricity might be produced *without*, or *with* chemical power. The rationale of these differences never has been given, unless by my theory, which supposes caloric to be present in the one case, but not in the other. The electric column was the fruit of De Luc's sagacious enquiries, and afforded a beautiful and incontrovertible support to the objections he made to the idea, that the galvanic fluid is pure electricity, when extricated by the voltaic pile in its usual form. It showed that a pile really producing pure electricity, is devoid of the chemical power of galvanism.

We are informed by Sir H. Davy, that when charcoal points in connection with the poles of the magnificent apparatus with which he operated, were first brought nearly into contact, and then withdrawn four inches apart, there was a heated arch formed between them in which such non-conducting substances as quartz were fused. I believe it impossible to fuse electrics by mechanical electricity. If op-

posing its passage they may be broken, and if conductors near them be ignited, they may be acted on by those ignited conductors as if otherwise heated; but I will venture to predict, that the slightest glass fibre will not enter into fusion, by being placed in a current from the largest machine or electrical battery.

I am induced to believe, that we must consider light, as well as heat, an ingredient in the galvanic fluid; and think it possible that, being necessary to vitality in animals, as well as vegetables, the electric fluid may be the vehicle of its distribution.

I will take this opportunity of stating, that the heat evolved by one galvanic pair has been found by the experiments which I instituted, to increase in quantity, but to diminish in intensity, as the size of the surfaces may be enlarged. A pair containing about fifty square feet of each metal, will not fuse platina, nor deflagrate iron, however small may be the wire employed; for the heat produced in metallic wires is not improved by a reduction in their size beyond a certain point. Yet the metals above mentioned, are easily fused or deflagrated by smaller pairs, which would have no perceptible influence on masses that might be sensibly ignited by larger pairs.—These characteristics were fully demonstrated, not only by my own apparatus, but by those constructed by Messrs. Wetherill and Peale, and which are larger, but less capable of exciting intense ignition. Mr. Peale's apparatus contained nearly seventy square feet, Mr. Wetherill's nearly one hundred, in the form of concentric coils, yet neither could produce a heat above redness on the smallest wires. At my suggestion, Mr. Peale separated the two surfaces in his coils into four alternating, constituting two galvanic pairs in one recipient. Iron wire was then easily burned and platina fused by it. These facts, together with the incapacity of the calorific fluid extricated by the



calorimotor to permeate charcoal, next to metals the best electrical conductor, must sanction the position I assigned to it as being in the opposite extreme from the columns of De Luc and Zamboni. For as in these, the phenomena are such as are characteristic of pure electricity, so in one very large galvanic pair, they almost exclusively demonstrate the agency of pure caloric."

The preceding facts, while most interesting, but otherwise explained to-day, had scarcely been published, when Hare wrote to a friend:

"I am constructing a galvanic apparatus, in a glass jar, two and a half inches in diameter, by eight inches in height, of *coils of copper and zinc*; the zinc plates are about nine inches by six, and are rolled up with the copper by means of a mandrel, and two pieces of soal leather interposed, one eighth inch thick, the copper beginning on the inside and ending on the outside; so that it takes fourteen inches of this metal. There will be eighty pairs only, at first. The soal leather is used merely to give them the proper spiral; and is, of course, withdrawn, when they are taken off the mandrel. Narrow pieces of wood are employed to keep them apart afterwards."

Whereupon the following epistle was sent to him:

"My dear Sir:

"Yale College, October 23, 1821

I was much impressed by your account of the Galvanic Deflagrator, and of the fine experiments which you performed with it. By means of your kindness in sending me your original apparatus, (the only one which, as far as I am informed, has hitherto been constructed) I had it in my power, early in the month of June, to repeat your experiments in my course of public lectures. Large numbers of intelligent persons attended, in addition to the classes, and the results gave great pleasure and satisfaction. My health being, at that time, very feeble, it was not in my power to

pursue the subject to the extent which I had intended, and expecting to resume it, I had postponed the writing of a notice of your instrument, hoping that by and by, I could do it more to my own satisfaction. But as no one else appears to have repeated your experiments, I have concluded, even at this late moment, to throw a hasty notice into the Journal, although it has not been in my power to add anything to the experiments performed in June.

I can say with truth that I consider your *Deflagrator* as the finest present made to this department of knowledge, since the discovery of the Pile by Volta, and of the trough by Cruickshank. The vessels being filled with the fluid before hand, prevents any haste or confusion, and the advantage which your arrangement gives the operator, of immersing, at one quick movement, the whole of an extensive series, is very great. Being perfectly ready, and with the poles in his hand, the teacher only giving a signal to his assistant to immerse the coils, instantly directs the whole power to the desired point, and produces results, which both in brilliancy and energy, totally surpass anything before effected by the same surface of metal, arranged in the same number of combinations. This will appear the more remarkable, when it is remembered that your apparatus produced these effects without insulation. Although, through your civility, I have just received the glass jars by which you insulate your coils, I have not yet been able to use them, and can therefore speak only of the results obtained without them.

With your eighty coils of fourteen inches by six, for the copper, and nine by six for the zinc, I obtained effects which, as to everything that related to intense heat and light, and brilliant combustion, far surpassed the powers of a battery of the common form of six hundred and twenty pairs of plates—one hundred and fifty pairs of which, of six inches square, are insulated by glass partitions—one hundred pairs



of the same size, and three hundred of four inches square, are insulated by resin and the rest, either by Wedgewood's ware or by resin, making in the whole a battery with a surface of thirty-six thousand eight hundred and eighty square inches. Yours has a surface of only twenty-two thousand and eighty square inches, *but even without insulation* it is incomparably more powerful than the other with that advantage. This is the most singular circumstance connected with your new apparatus, and which goes far to shake our previous theoretical opinions, if not to support your own.

I repeated every important experiment stated in your memoir, and with results so similar, that it is scarcely necessary to relate them. The combustion of the metals was brilliant beyond everything which I had witnessed before, and the ignition of the charcoal points was so intense, as to equal *the brilliancy of the sun*; The light was perfectly intolerable to eyes of only common strength. If I were to name any metallic substance which burned with more than common energy, it would be a common brass pin, which, when held in the forceps of one pole, and touched to the charcoal point on the other, was consumed with such energy, that it might be said literally to vanish in flame.

The light produced between the charcoal points when immersed beneath acids, oils, alcohol, ether, water, &c. was very intense, and platina melted in air as readily as wax in the blaze of a candle. It is a very great advantage of your *Deflagrator* that we can suspend the operation at any moment, with the same facility with which it was commenced. A look, directed to the assistant, is sufficient to raise the coils out of the fluid. All action instantly ceases, neither the metal nor the fluid are wasting any farther, and the lecturer is therefore at ease while he illustrates and reasons, and when he is ready and not before, he proceeds to his next experiment. In the meantime, the instrument, during a certain

period, rather gains than loses strength, by the raising of the coils. It seems as if the imponderable fluids, partially exhausted from it by its continued action, had time again to flow in from surrounding objects, and thus to impart new energy. I found the power of the instrument to last for several days, although declining, and the same charcoal points, when well prepared, would also continue to operate for several days. When the coils, after immersion, had been suspended, for some hours, in the air, a coating of green oxid or carbonat of copper always formed on one part of the outside of the copper coils, and on the same part in all, but no where else. If I do not misremember, it collected next to the negative pole, but was, of course, always removed by the next immersion, though it was formed again at the next suspension.

One circumstance occurred during these experiments, which demands farther attention.

In the hope of uniting the power of your *Deflagrator*, with that of the common galvanic battery, I connected your instrument with the powerful one mentioned above. Both instruments, *when separately used*, acted, *at the time*, with great energy, producing both their appropriate and common effects, in a very decided manner; but, on connecting by the proper poles, the battery of six hundred and twenty pairs, with the *deflagrator* of eighty coils, I was greatly surprised and disappointed, at finding the power of both instruments so completely paralyzed, that, at the points where a moment before, and when separate, a stream of light and heat, hardly to be endured by the eye, was poured forth—now, when connected, both instruments could scarcely produce the minutest spark. On separating the instruments, they both resumed their activity; on again connecting them, it was again destroyed, and so on, as often as the experiment was made. While they were in connexion, provided the coils were lifted out of the acid, so as to hang in the air merely,



then the power of the common galvanic battery would pass through the *Deflagrator*, which appeared to act simply as a conductor, and as might have been expected, when so extensive a conductor was used, the power of the common battery was, in this case, considerably diminished, while that of the *Deflagrator* did not act at all.

If, while things were in this situation, the coils of the *Deflagrator*, without being plunged, were lowered so far as merely to dip their inferior extremities, say only one fourth of an inch in the acid, the communication was immediately arrested, and all effect destroyed almost as completely, as when the coils were wholly immersed. Thus it appears that the inability to act, in connexion with the common galvanic battery, depends upon the relation of the fluid and metal, and not upon that of the metals merely. These experiments should be repeated, with the aid of the insulating glasses, placed so as to receive the coils of your machine. I should be very curious to know whether the effects would be the same; and as I now have the glasses, I shall as soon as possible, try this experiment. We must look to you, Sir, for the explanation of this singular incompatibility between the two instruments. At present, I confess myself unable to explain it. It may, very possibly, lead to important results, and may have a bearing, such as I have not now time to discuss, on your own peculiar theory.

I would state that the mode of connecting the two batteries was varied in every form which occurred, not only to myself, but to several able scientific gentlemen, who were present at these experiments, and who were equally with myself surprised and confounded by their results.

I congratulate you upon the brilliant additions which you have made to our experimental means, in this department of knowledge; along with your invention of the compound blowpipe, they fairly entitle you to the gratitude of the scien-

tific world, notwithstanding the uncandid attempts which, in relation to the blowpipe, I am sorry to see, are still persevered in, to deprive you of the credit which you so richly deserve.

I remain, as ever, your friend and servant,

B. SILLIMAN."

In reply to which Hare wrote:

"My dear sir:

"Philadelphia, Nov. 5, 1821.

I have received your letter on the *Deflagrator* which I sent you last spring. I fear you have done me more than justice.

I should not be surprised, if the coils when insulated by the glass jars, should form a circuit with your other apparatus, better, than when immersed in the troughs. You will observe that when recently lifted from out of the acid, the air insulates the coils; while the pieces of wood used to keep the copper from touching the zinc, act to a certain extent like the moistened cloth in Volta's original pile.—When in this situation, the poles will affect an electromotor much more powerfully, than when the coils are immersed; though in one case, the igniting power will burn a platina wire of one eighth of an inch in thickness, in the other it will not burn Dutch gold leaf.

In my memoir, on a new theory of galvanism, is the following passage: 'According to my view, caloric and electricity may be distinguished by the following characteristics. The former permeates all matter more or less, though with very different degrees of facility. It radiates through air with immeasurable celerity, and distributing itself through the interior of bodies, communicates a reciprocally repellent power, to atoms, but not to masses. Electricity does not radiate in or through any matter, and while it pervades some bodies, as metals, with almost infinite velocity; by others it



is so far from being conducted, that it can pass through them only by a fracture or perforation. Distributing itself of choice over surfaces only, it causes reaction between masses, but not between the particles of the same mass. The disposition of the last mentioned principle (electricity) to get off by neighbouring conductors, and of the other (caloric) to combine with the adjoining matter or to escape by radiation, would prevent them from being collected at the positive pole, if not in combination with each other. Were it not for a modification of their properties consequent to some such union, they could not, in piles of thousands of pairs, be carried forwards through the open air and moistures, the one so well calculated to conduct away electricity, the other so favourable to the radiation of caloric.

Pursuing the same subject in a subsequent memoir, also published in your Journal, I thus expressed myself, 'As yet no adequate reasons have been given why, in operating with the pile, it is not necessary, as in the process of Van Marum and Wollaston, to enclose the wires in glass or sealing wax, in order to make the electricity emanate from a point within a conducting fluid. The absence of this necessity is accounted for, according to my hypothesis by the indisposition which the electric fluid has to quit the caloric in union with it, and the almost absolute incapacity which caloric has to pass through fluids unless by circulation. I conceive that in galvanic combinations, electro-caloric may circulate through the fluids from the positive to the negative surface, and through the metal from the negative to the positive. In the one case caloric subdues the disposition which electricity has to diffuse itself through fluids, and carries it into circulation. In the other, as metals are excellent conductors of caloric, the prodigious power which electricity has to pervade them agreeably to any attractions which it may exercise operates almost without restraint. This is fully exemplified in my galvanic

deflagrator, where eighty pairs are suspended in two recipients, forty successively in each, and yet decompose potash with the utmost rapidity, and produce an almost intolerable sensation when excited only by fresh river water. I have already observed that the reason why galvanic apparatus composed of pairs consisting each of one copper and one zinc plate, have not acted well without insulation; was because electro-caloric could retrocede in the negative, as well as advance in the positive direction.'

Agreeably to these views, in order to prevent the escape of the electricity put into motion by the series, the caloric must bear a certain proportion to it. It is to be inferred, consistently with the same hypothesis, that this proportion did not exist in the series which you connected with the deflagrator. The fluid presented to the latter had too much electricity in it; and hence instead of passing into circulation, escaped. When the coils were suspended in air, this escape was less favored than when they were covered by the diluted acid.

Faithfully yours,

ROBERT HARE."

Many of the minor communications, sent from time to time to Silliman by Hare, possessed more than common value. For instance, on one occasion he told how he had infused alcohol with alkanet root, when to his astonishment the solution instead of being red was blue in color. It occurred to him that the alcohol had stood over pearlash, so a second solution with pure alcohol was promptly made. The tincture was red in color, which was rendered blue by a drop of an alkaline solution. So he proposed to use alkanet in place of litmus. "The alkanet infusion must be made blue by an alkali and restored by an acid, instead of being as in the case of litmus reddened by an acid, and restored by an alkali. Thus as the one is indirectly a test for alkalies, so is the other for acids. In making the infusion of alkanet blue for this



purpose, the smallest quantity of alkali should be used, which will accomplish the change, as in that case less acid will be requisite to restore the color, and thus manifest its presence in any solution to be tested."

He also observed that the silver crystals which form spontaneously when a silver coin is dissolved in nitric acid, diluted no more than is necessary for the solution to proceed actively, give no trace of copper when it is redissolved. He wondered whether this would not be a "good preliminary step in refining silver, or for getting the nitrate for lunar caustic or as a test." He tells, too, that upon saturating strong nitric acid, gotten from dry niter, with ammonium carbonate in a retort, the resulting salt was procured in a compact form and upon distillation forthwith yielded nitrous oxide. He used as containing vessels for the gas, bags of leather soaked with boiled linseed oil.

An exceedingly important correspondence between the two friends grew out of Hare's discoveries; indeed, so interesting are the letters that no account of Hare's life would be complete which omits them. The following are noteworthy:

Hare to Silliman:

"My dear Sir:

"Phila. March 5, 1822.

In reply to your enquiries on the subject of the *Calorimotor*, and the expediency of employing one during your lectures, it may be proper to mention, that the phenomena produced by it are more agreeable to the eye and therefore more popular, than any which can be performed without greater difficulty. By the time the Calorimotor is completely immersed in the acid solution, the wire in the forceps is rendered white hot, and takes fire, emitting the most brilliant sparks. In the interim, an explosion usually gives notice of the extrication of hydrogen in a quantity adequate to reach the burning wire. Immediately after the explosion,

the hydrogen is reproduced with less intermixture of air, and rekindles, corruscating from among the forty interstices, and passing from one side of the machine to the other in opposite directions, and at various times, so that the combinations are innumerable. The flame assumes various hues, from the solution of more or less of the metals, and a blazing froth, rolls over the sides of the recipient. When the calorimotor is withdrawn from the acid solution, the surface appears for many seconds like a sheet of flaming foam.

I refer you to the last paragraph of my memoir on the *Deflagrator*, for some results obtained by calorimotors, of different sizes, which I deem to be scientifically important.

With respect to the comparative powers of concentric coils, of copper and zinc and of plates of those metals alternating; if only a few pairs are to be employed, I believe it a matter of indifference which construction we adopt. I have, however, found to my cost that it is far from being so when the series is numerous. Last summer I constructed an apparatus of one hundred pairs, each containing six alternated plates, three of each metal. On trial, it proved much less powerful than the *Deflagrator* sent to you, though the zinc surface in each pair, was one seventh larger, and the number of the series one fourth more extensive. The exposure to each other, of the copper and zinc plates terminating the different pairs, struck me as disadvantageous. I therefore, removed the external zinc plate in each, so that the pair afterwards, consisted severally of three copper and two zinc plates, and were bounded by copper towards both poles. There was some comparative gain by this change, as the power was not lessened in proportion to the diminution of zinc surface. Still the result was unsatisfactory. I then had some boxes made with partitions of glass, to be interposed between the pairs of the series. These were employed as is usual with galvanic troughs, made with partitions, ex-



cepting the deficiency of bottoms, and their being suspended to the beams, so as to be simultaneously immersed with the galvanic surfaces which they were intended to insulate. The power of the series was not amended by this contrivance. It had often occurred to me, that surrounding the zinc by copper, might be an indispensable feature in the arrangement of my Deflagrator of coils. In order to test the correctness of this surmise, I proceeded to form an apparatus of pairs, each consisting of a case of copper, containing one zinc plate of seven inches by three, the size used, in the apparatus above described. In these pairs, as in those contrived by Wollaston, the edges of the zinc were supported by grooved pieces of wood passing between them and the copper. There was, however, this apparently slight, but really important difference, that the cases employed by me, were open at top and bottom, instead of exposing the edges of the zinc laterally, as in Wollaston's. One hundred galvanic pairs, thus made, were suspended to two beams, each holding fifty. Between each case, a piece of pasteboard soaked in shell lac varnish, was interposed; so that the whole constituted a compact mass, into which a fluid could not enter, unless through the interstices purposely preserved between the copper and zinc. The phenomena produced by this apparatus, on immersion, were upon the whole more interesting than those produced by my original deflagrator; especially in the length of the jet between the poles, and the power of permeating charcoal. Yet the apparatus was comprised within one eighth of the space, and is not (in oxidizable superficies) of half the extent.

Having added three more beams, of fifty pairs each, to my apparatus, I found the power increased fully in the ratio of the number. You know that my eyes are naturally very strong. The light produced by the compound blowpipe, though I operated without glasses, only dazzled them for a

time, and thitherto I had felt no other inconvenience from my galvanic experiments. Rendered thus bold by previous immunity I still dispensed with the annoyance of spectacles. In consequence, my eyes, after operating with the last mentioned series of two hundred and fifty, were on the following day so much inflamed, as to be blood shot, and painfully susceptible of the day light. The judicious application of twenty leeches to each of the eye-lids, pursuant to the advice of my friend, Dr. Dewees, afforded me surprising relief, and my eyes are now well enough to finish this letter, though a few days since when I began it, I was under the necessity of employing an amanuensis.

By this series of 250, Barytes was deflagrated; and the Platina which supported it destroyed like pasteboard before an incandescent iron. A platina wire three sixteenths of an inch in thickness, was made to flow like water. Iron of like dimensions burned explosively. When the experiments were repeated before my class of more than three hundred pupils, and many visitors, there were very few who could bear the light with the naked eye.

Much attention was excited by the deflagration of a stream of mercury. This was accomplished in the following way. A wire proceeding from one pole of the deflagrator, was introduced into some mercury held in a glass basin; and another wire proceeding from the other pole, into some mercury in another vessel, having a capillary orifice which might be closed by the finger or a stopple. This last mentioned vessel with the mercury running from it was supported at such a height above the surface of the mercury in the glass basin, as to permit the discharge to take place through the metallic stream just as the galvanic surfaces were subjected to the acid. The mercury deflagrated explosively.

The experiments may be varied, by causing the stream of mercury to fall on iron filings, or card teeth.



When the phenomena of a series of 250 pairs of 7 inches by 3, are such as I have described what would be the power of a deflagrator with plates, as large as Children's, and as numerous as Davy's?

Probably the most useful mode of applying such instruments to analysis, would be to expose substances to the discharge in vacuo on carbon. I observed that after iron and charcoal were ignited between the poles during a few seconds, under an exhausted receiver, on admitting the air, a flash took place, and a yellowish red fume appeared which condensed on the glass. It would seem the iron was volatilized, and that the admission of air oxidized the vapour.

A deflagrator of 250 or 300 pairs is found to produce torture when applied for a short time to the back of the hand, and it is difficult for the sufferer to believe, that his skin has not been cauterized. One of my pupils showed me a slight excoriation, which he considered as arising from it, on the spot where the positive pole had touched him. Between the excitement of acid, and water, the difference of power in affecting the flesh, is far less than with metals, charcoal or potash. Upon these substances, the excitement by water has no influence, but to the sensation is painful, though it may be borne longer, than when acid is used. Neither is the shock greater, in any sensible degree, at the moment of immersion, than afterwards. The effect upon the electrometer, is at least as great, with water, as with acid. Immediately over any of the most turgid veins, where the skin is tender, as on the back of the hand, will be found the greatest sensibility. The positive pole, is most capable of producing pain. This I had frequent opportunities of ascertaining, by the observations of those who, now knowing how to distinguish it from the negative pole, could not have been biassed in their opinion. Upon a common gold leaf electrometer, a deflagrator of 300 pairs will have no influence. I have con-

structed one by means of a bottle, a single slip of gold leaf, and a knob at right angles to it, supported by a screw, so as to be easily moved nearer to or further from the leaf. The wire from which the latter is suspended, passes through a cork in the neck of the bottle. The screw enters through a nut, cemented into a hole drilled on one side. When the wire which supports the leaf, is fastened to one of the poles, every time the screw is touched by the other the leaf will strike the ball provided the distance be very small, perhaps not greater than the tenth of an inch. This result was obtained at a greater distance when the coils had been recently withdrawn from the acid, than when they are covered by it. I have known a piece of dry sealing wax, as big as a chestnut, without friction, to affect this electrometer as much as my largest deflagrator.

A magnetic needle was very powerfully disturbed by the deflagrator, under all its forms. The celerity with which the galvanic surfaces may be immersed in, or withdrawn from the acid, contributes much to economy, and to the ease of the operator in galvano-magnetic enquiries.

The prevalent notion, that the intense light and heat produced by galvanic action, are results secondary to electricity, the presence of which is at times only indirectly discoverable, the more surprises me; since it does not in the smallest degree, elucidate the primary operation, by which this principle is alleged to be evolved. According to some philosophers, the contact accompanied by their solution, evolves electricity in quantity sufficient to extricate heat and light from a wire made the medium of transmission. They do not, however, explain why the electricity does not, according to all its known habitudes, rapidly escape through the water, as fast as generated, instead of proceeding from one plate to another, in order to pass off through a second portion of the same fluid. Would it not be more philosophical to suppose that the heat and light result *directly* from the



causes supposed to produce them *indirectly*; especially, as we actually see *them* in a high degree of intensity, while the characteristic agency of the principle, by which they are supposed to be produced, is but feebly perceived, or imperfectly demonstrated? In the case of a single galvanic pair, electricity has never been alleged discoverable, unless by the questionable assistance of condensers.

Besides, without supposing caloric and light to circulate from the apparatus through the conjunctive wire, those who consider them as material, will find it impossible to account for the durability of the ignition. If it be supposed that these principles are extricated from the metal, only by electricity passing through it, their repeated or incessant expenditure, ought sooner or later to exhaust the metal, and render it incapable of further ignition.

On this subject, especially, as connected with magnetism, and mechanical electricity, you shall hear from me again.

R. H."

"Yale College, New Haven,

"April 9th, 1822.

"My dear Sir:

In my letter of October 23, 1821, addressed to you respecting the experiments which I had performed with your deflagrator, I mentioned the incompatibility which I discovered to exist between your apparatus and the common galvanic battery. I have recently repeated these experiments with some additions and variations which I now take the liberty of stating to you.

In the trials made last October with your instrument, the coils were used without glasses, being immersed in a fluid contained in a common recipient in those recently performed, and which I shall now relate, the metallic coils were individually insulated, for they were immersed in the cylindrical glasses belonging to the apparatus, it being previously

connected with the common galvanic battery by its proper poles as described in my former letter; the effects were however in no respect different from those before observed, so that the insulation of the coils appears to be a fact of no importance. In the first experiment the deflagrator being connected by its proper poles with a galvanic battery of 300 pairs of four inch plates cemented in mahogany troughs, and interposed between the two rows of the deflagrator, of forty coils each, lost all its power, and the effect produced was very much inferior to that of the battery alone, for in fact the spark was hardly perceptible.

The chemical or decomposing powers of the common galvanic battery, were also found to be suspended by the connexion—for the 300 pairs which usually decompose water, salts, &c. with decisive energy, now produced in water scarcely a bubble of gas, and hardly affected dilute infusion of purple cabbage. The power of giving a shock was also destroyed by the connexion.

When the coils were raised out of the fluid and suspended only in the air, they acted as conductors of the power of the common battery, which now produced all its appropriate effects, although, even in this case, the galvanic influence appeared somewhat diminished, which would of course arise both from *the extent* of the conducting surface, and from the fact that a part of the substance, namely, the wedges of moist wood, interposed between the metals was an imperfect conductor.

These experiments (including the former trials) were made with different combinations from 620 pairs down to 20, and were attended, uniformly with the same result; viz: an almost entire suspension of the power of both instruments.

In one of the experiments, twenty-five pairs of the zinc and copper plates, six inches square, connected by slips of copper and suspended from a beam of wood were immersed



in a trough without partitions filled with an acid liquor, and the connexion being formed with the deflagrator, the power of the latter instrument was found to be completely destroyed—a similar result was obtained by a battery consisting of fifty *triads* of plates two inches square, each zinc surface being coated by a copper plate after the manner of Dr. Wollaston—the object of this arrangement was, to ascertain, whether a battery, in which the arrangement of metals was similar, to that in the deflagrator, would produce a result in any respect different from that of the common battery; the effect however was precisely the same. In most of the experiments the connexion of the poles was occasionally reversed. This circumstance however made no difference in the result. A feeble spark was obtained as before. Every thing tended to countenance the opinion that the interposition of the common galvanic battery operated simply as an impediment—that it was completely inert in relation to the deflagrator, and the deflagrator in relation to it,—that the power of neither would pass through the other, and consequently that each was to be regarded, with respect to the other, simply as so much interposed matter, constituting a conductor more or less imperfect. To bring this conjecture to a decision, the number of interposed plates was constantly diminished, until the connexion was formed by no more than twenty pairs. In this state of things, the power of the deflagrator passed freely, although somewhat diminished. The connexion was now formed with smaller and smaller number of pairs; the activity of the deflagrator in the mean time rapidly increased, until the moment, when only one pair was employed (this pair being, however, like the others, immersed in an acid fluid), then there was no perceptible impediment, and the effect was as brilliant as when nothing was interposed.

I have thought these curious facts worthy of being preserved, and I have addressed them to you with the hope that

you will be able to throw some light upon this singular anomaly, which to me appears to be incapable of explanation, in consistency with the received theories of galvanism. Hoping that you will, through the medium of the journal, favour the public with your views upon this subject

I remain with very great respect,

Your friend and servant,

B. SILLIMAN."

Again, Silliman to Hare:

"Yale College, New Haven,

"May 10, 1822.

"My dear Sir,

In your memoir on your Galvanic Deflagrator, when speaking of the ignition produced by that instrument, in charcoal points, you remark: 'If the intensity of the light, did not produce an optical deception, by its distressing influence upon the organs of vision, the charcoal assumed a pasty consistence, as if in a state approaching to fusion.

'That charcoal should be thus softened without being destroyed by the oxygen of the atmosphere, will not appear strange, when the power of galvanism in reversing chemical affinities is remembered; and were it otherwise the air could have no access, first because of the excessive rarefaction, and in the next place as I suspect on account of the volatilization of the Carbon, forming about it a circumambient atmosphere. This last mentioned impression arose from observing, that when the experiment was performed in vacuo, there was a lively scintillation, as if the Carbon in an aeriform state, acted as a supporter of combustion on the metal."

This paragraph, at the time of perusing it, excited in my mind a lively interest, and a strong wish to see so fine a result, as the fusion of charcoal, confirmed by an experiment admitting of no question. What you threw out by way of surmise, and without positively affirming it, I think I am now able to substantiate.



During the three last weeks of March, I was much occupied with your deflagrator. The medium of communication, between the poles, was generally, charcoal prepared for the purpose, by intensely igniting pieces of very dry mahogany, buried in a crucible, beneath white siliceous sand. The pieces of charcoal thus prepared, were about half an inch in diameter, and from one and half inch, to three inches in length; they were made, as usual, to taper to a point, and the cylindrical ends were placed in the sockets connected with the flexible lead tubes, which form the polar terminations of the series.

The metallic coils of the deflagrator, being immersed, on bringing the charcoal points into contact, and then withdrawing them a little, the most intense ignition took place, and I was surprised to observe that the charcoal point of *the positive pole*, instantly *shot out*, in the direction of the longer axis, and thus grew rapidly in length; it usually increased, from the 10th to the 8th of an inch, and in some instances attained nearly  $\frac{1}{4}$ th of an inch in length, before it broke off and fell. Yesterday and to-day, I have carefully repeated these experiments, and in no instance, has this shoot from the positive pole failed to appear. It continues to increase rapidly, as long as the contiguous points of charcoal are held with such care, that they do not strike against each other. When they impinge with a slight shock, then the projecting shoot or knob breaks off and falls, and is instantly succeeded by another. The form of the projecting shoot, is sometimes cylindrical, but more generally it is that of a knob, connected with the main piece of charcoal, by a slender neck, much resembling some stalagmites. It is always a clear addition to the *length* of the charcoal, which does not suffer any waste except on the parts, *laterally* contiguous to the projecting point.

The charcoal of the negative pole, in the mean time, undergoes a change precisely the reverse. Its point instantly dis-

appears, and a crater-shaped cavity appears in its place; it suffers a rapid diminution in the direction of its length, and immediately under the projecting and increasing point of the positive pole; but it is not diminished, or very little, on the parts laterally contiguous. If the point of the positive pole be moved over the various parts of the contiguous negative charcoal, it produces a crater-shaped cavity over every place where it rests, for an instant. In every repetition of the experiment, (and the repetitions have been numerous,) this result has invariably occurred. *It appears as if the matter at the point of the negative pole was actually transferred to the positive, and that the accumulation there, is produced by a current flowing from the negative to the positive, or at least by an attraction exerted in that direction, and not in the other.* It does not appear easy to reconcile this fact with any electrical or igneous theory.

In order to ascertain whether the projection of the charcoal at the positive pole was caused by an actual transfer of carbon from the negative, a piece of metal was substituted for the charcoal at the negative pole, and when the two were brought into contact, the charcoal point of the positive pole remained unaltered in form, although a little shortened by the combustion. The experiments with the two charcoal points were varied by transferring, that at the positive end (and on which a projection was already formed) to the opposite pole, and that at the negative, and in which a corresponding cavity appeared, to the positive.

The result was, that the cavity now placed at the positive pole, disappeared, and was immediately seen at the negative, while the projection, now placed at the negative pole, was transferred to the positive. These experiments were several times repeated, and uniformly with the same result. They seem to leave no doubt, *that there is a current from the negative to the positive pole, and that carbon is actually trans-*



*ferred by it in that direction; if transferred, it must probably be in the state of vapour, since it passes through the ignited arch of flame, which is formed when the points are withdrawn a little distance; when it arrives at the positive pole. It there concretes in a fluid, or at least in a sort of "pasty" state.*

But the most interesting thing remains yet to be stated. On examining with a magnifier, the projecting point of the positive pole, it exhibited decisive indications of having undergone *a real fusion.*

The projecting point or knob, was completely different from the charcoal beneath. Its form was that of a collection of small spheres aggregated; exhibiting perfectly, what is called in the descriptive language of Mineralogy, botryoidal or mammillary concretions. Its surface was smooth and glossy, as if covered with a varnish; the lustre was metallic, the colour inclining to grey, exhibiting sometimes iridescent hues, and it had entirely lost the fibrous structure. In short, in colour, lustre, and form, the fused charcoal bore the most striking resemblance to many of the beautiful stalactical and botryoidal specimens of the brown hæmatite. The pores of the charcoal had all disappeared, and the matter had become sensibly harder and heavier.

I repeated the experiments, until I collected a considerable quantity of these fused masses; when they were placed contiguously, upon some dark surface, with some pieces of charcoal near them, they appeared when seen through a magnifier, so entirely different from the charcoal, that they would never have been suspected to have had any connexion with it, had it not been, that occasionally some fibres of the charcoal adhered to the melted masses. The melted and unmelted charcoal, differ nearly as much in their appearance as pumicestone and obsidian, and quite as much as common stones do, from volcanic scorïæ, excepting only, in the article of colour. It is to be understood, that the examination, is in every in-

stance, made by means of a good magnifier, and under the direct light of the sun's rays, as the differences are scarcely perceptible to the naked eye, especially in an obscure light. The portions of melted charcoal, are so decidedly heavier than the unmelted, that when fragments of the two of a similar size are placed contiguously, the latter may be readily blown away by the breath, while the former will remain behind, and when the vessel containing the pieces is inclined, the melted pieces will roll with momentum, from one side to the other in a manner, very similar to metallic substances, while the fragments of charcoal will either not move, or move very tardily.

It should be observed, that during the ignition of the charcoal points, there is a peculiar odour, somewhat resembling electricity, and a white fume rises perpendicularly, forming a well defined line above the charcoal. There was also, a distinct snap or crackling when the two points were first brought together.

Wishing to ascertain whether the Alkali, present in the charcoal, had any effect in promoting the fusion, some pieces of prepared charcoal were thoroughly boiled in water, and were then again exposed to a strong heat in a furnace beneath sand in a crucible. These pieces when connected in the circuit exhibited the same appearances as the other and proved equally fusible.

Without destroying cabinet specimens, I could procure no diamond slivers, and have not therefore, attempted the fusion of the diamond, which must be left to another opportunity. Our circle of fusible bodies, so much enlarged by the use of your instruments, is now so nearly complete, that it would be very desirable to fill the only remaining niche, namely, that occupied by plumbago, anthracite, and the diamond.

I remain as ever, truly, your friend and servant,

B. SILLIMAN."



“P. S. I do not suppose, that those who repeat these experiments, will succeed with the common galvanic apparatus. I deem it indispensable, that they be performed with the deflagrator, and with one equal in power to mine.”

And what the great experimenter had to say will be found in the appended letters:

“My dear sir,                      “Philadelphia, May 25th, 1822.

In a former letter you mentioned, that you had found the power of the galvanic deflagrator, when its coils were subjected to acid in troughs without partitions, incompatible with the power of other voltaic series, of the usual forms; that when associated with them in one circuit, it could neither give, nor receive excitement. You now inform me, that this incompatibility is not lessened when the coils are insulated by glass jars. It follows, that electrical insulation has less influence on the action of this instrument, than I had supposed, and it of course confirms my idea, that the deflagrating power is not purely electrical.

It cannot be doubted, notwithstanding your experiments, that there is a principle of action, common to the various apparatus which you employed, and all other galvanic combinations. The effect of this principle of action, however, varies widely according to the number of the series, the size of the members severally, and the energy of the agents interposed. Towards the different extremes of these varieties are De Luc's Column apparently producing pure electricity, and one large galvanic pair, or calorimotor of two surfaces, producing, in appearance, only pure caloric. At different points between these, are the series of Davy and Children; the one gigantic in size, the other in number. In the deflagrator we have another variety, which, with respect to size and number, is susceptible of endless variation.

It must be evident that no galvanic instrument where a

fluid is employed, could aid, or be aided by, the columns of De Luc or Zamboni, nor could the influence of either be transmitted by the other. A calorimotor could not aid Davy's great series; nor could the latter, act through a calorimotor. Taking it for granted that there can be no oversight in your experiments, this incompatibility of exciting power must exist to a great degree, under circumstances where it could hardly have been anticipated.

Were the fluid evolved by galvanic action purely electric, the effect of batteries of different sizes, when united in one circuit, ought not to be less than would be produced if the whole of the pairs were of smaller size. But if on the contrary, we suppose the voltaic fluid compounded of Caloric, light and electricity, so obviously collateral products of galvanic action; the ordinary voltaic series employed in your experiments, may owe its efficacy more to electricity—and the deflagrator more to caloric. The peculiar potency of both may be arrested when they are joined, by the incompetency of either series to convey any other compound than that which it generates. The supply of caloric from the ordinary series may be too small, that of electricity too large; and vice versa. It might be expected that each would supply the deficiency of the other; but it is well known that many principles will combine only when they are nascent. The power of my large deflagrator in producing decomposition, is certainly very disproportional to its power of evolving heat and light. When wires proceeding from the poles were placed very near each other under water, it was rapidly decomposed; but when severally introduced into the open ends of an inverted syphon, filled with that fluid, little action took place: Potash is deflagrated and the rosy hue of the flame indicates a decomposition. Still however the volatilization of the whole mass, and intense ignition of the metallic support, prove that the calorific influence is greatly and peculiarly predominant.



I fear that in my essays on galvanic theory, the possible activity of light, has been too much overlooked. The corpuscular changes which have been traced to the distinctive energies of this principle, are so few that we have all been in the habit, erroneously perhaps, of viewing it as an inert product in those changes, effected by caloric, electricity and chemical action, which it most strikingly characterizes. Yet reflecting on the prodigious intensity in which it has been extricated by the deflagrator, it seems wrong not to suspect it of being an effective constituent of the galvanic stream. Possibly its presence in varying proportions, may be one reason of the incompatibility of the voltaic current as generated under different circumstances, or by various forms of apparatus. It may also suggest, why in addition to changes in the force of nature of the sensation produced by the galvanic discharges which may be considered as dependent on electric intensity, peculiarities have been observed, which are not to be thus explained. The effect on the animal frame, has been alleged to be proportional to the electrical *intensity*, the effect on metals to the *quantity*; but according to the observations of Singer (which are confirmed by mine) the electrical intensity is as great, with water as with acid, if not greater even than with the latter. The reverse is true of the shock. When the plates of the deflagrator are moistened, and withdrawn from the acid, the shock is far less powerful; yet the electrical excitement appears stronger. Light is undeniably requisite to vegetable life, perhaps it is no less necessary in the more complicated process of animal vitality, and the electric fluid may be the mean of its distribution. The miraculous difference observed in the properties of organic products, formed of the same ponderable elements, may be due to imponderable agents conveyed and fixed in them by galvanism. Hence it may arise, that the prussic acid instantaneously kills when applied to a tongue, containing the same

ponderable elements. When by the intense decomposition of matter, light is always evolved; when an atom of tallow gives out enough of it to produce sensation in the retina of millions of living beings why may it not when presented in due form, influence the taste, and otherwise stimulate the nervous system. For such an office its substibility would seem to qualify it eminently. The phenomena of the firefly and the glow worm prove that it may be secreted by the process of vitality.

The discovery of alkaline qualities, as well as acid, in organic products whose elements are otherwise found, whether separate or in combination, without any such qualities, and the opposite habitudes of acids and alkalies with the voltaic poles, and their power of combining with, and neutralizing each other, indicate that there may be something adventitious which causes alkalinity and acidity, and that this something is of an imponderable character, and dependent on galvanism.

In the number of your Journal for October last, I gave my reasons for believing in the existence of material imponderable principles, producing the phenomena of heat, light and electricity. The co-existence of these principles in the medium around us, their simultaneous, or alternate agency and appearance, during many of the most important processes of nature, seem to me to sanction a conjecture, that as ingredients in ponderable substances they may cause those surprisingly active and wonderfully diversified properties usually ascribed to apparently inadequate changes, in the proportions of ponderable elements.

In obedience to your request, I have thus displayed the ideas at present awakened in my mind by these obscure and interesting phenomena. I am not willing to assume any responsibility for the correctness of my conjectures. Possibly they may excite in you farther and more correct speculations."



Berzelius (1822) wrote a most complimentary note to Silliman upon his experiments with the deflagrator, taking occasion also to add:

“The discordance of the ordinary pile with the Deflagrator appears inexplicable to me, except by the theory of Mr. Hare, which though ingenious, I find it difficult to admit, since the electromagnetic phenomena are in all their characters the same as the ordinary electricity.”

“Red Lion 12 miles from Philad<sup>a</sup>

“June 20<sup>th</sup> 1822

“My dear Silliman:

I am thus far on my way to Providence—One of the last things which I did was to essay a calorimotor constructed for you. Its performance was superior to any I have before used—I presume it is now under way to New York, whence the Capt<sup>n</sup> is to ship it by one of your packets to New Haven.—This instrument was made of sheet zinc of double the thickness of that which I used, & of course it is more than four times as valuable.—But as I did not allow for this in my estimate the cost will be sixty five, instead of fifty dolls. The metals alone cost forty. I concluded however to send it. You may exhibit it to your class & the public; & afterwards return it if you please paying expenses.

I have observed the appearances with charcoal of which you spoke in your last communication: the protruding nipple on the zinc pole & cavity in the copper pole.

I have also repeated at different times & with different deflagrators the experiment of decomposing water by iron wires & found invariably gas to be given off at the zinc pole & oxide found at the copper pole—I hope soon to see you however & talk over these things in person.

Dr Chapman is about publishing in his journal the whole of our correspondence—

I am as ever

Yours

R. H.”

“ I shall probably be in New York when the Calorimotor reaches that Place—You may write to me there—You must have sustain'd a great shock in the loss of your Colleague Fisher.”

And in his textbook of 1831, Silliman comments on the peculiar power of the deflagrator thus:

“ 1. *Both in producing ignition and combustion, the deflagrators far surpass any other form of galvanic instruments.*

(a) *Charcoal points, two inches long, were, in the earliest experiments, instantly ignited, and the light surpassed that from any other source; it sometimes flashed through the windows upon the neighbouring buildings, and it has produced dangerous inflammation in the strongest eyes.*

(b) *At the moment of contact, or of very near approximation, a sharp rushing noise is heard, which is constantly renewed at certain distances, and is occasioned, evidently, by the passage of the electrical, calorific, and gaseous current.*

(c) *The existence of a current, from the positive to negative pole, is decisively proved by the transfer of the charcoal, from the positive to the negative pole; on the negative side, it rapidly collects into a knob, or projecting cone, or cylinder, which frequently becomes half an inch or more long, before it falls and gives place to another.*

(d) *On the positive pole a correspondent cavity is formed, out of which the vaporized matter rises and collects upon the negative pole; and a new cavity can be at any moment formed in the positive charcoal, by directing the negative point to a new place upon it; the cavities have no appearance of fusion, but retain the fibrous structure of the charcoal.*

(e) *If the charcoal points are now changed, that of the negative side retaining the projecting knobs, the latter will be immediately transferred to the other pole, whose corre-*



sponding *cavity will be soon filled* by the matter vaporized from the knob and after it is removed a cavity will come in its place, and thus the knob and cavity may be made, at pleasure, to exchange places.

(f) If a metallic wire be fixed in the positive pole, then there is no knob formed on the negative charcoal.

(g) These facts, which I first observed in 1821-22, are much less distinctly seen with a common battery, and not at all with one of moderate size, but they constantly occur, conspicuously, with a powerful deflagrator, and have been noticed by Dr. Hare, Dr. Griscom, Dr. Torrey, and several other gentlemen in this country. They were amply confirmed by Despretz.

(h) *The accumulation upon the negative pole has every appearance of fusion, after previous volatilization; it is in shining round masses, aggregated often like a cauliflower; it has a semi-metallic appearance; it is harder than the charcoal, heavier, much less combustible, and burns away slowly when ignited in air or with chlorate of potassa, and forms carbonic acid. It is obviously derived from the charcoal and must of course contain its impurities.*

(i) *Similar appearances are produced by plumbago and to a degree by anthracite; plumbago may be volatilized and accumulated upon charcoal, and the latter may be transferred to the former, when it exhibits beautiful tufts. The light from plumbago points is very intense and even more rich than from charcoal.*

2. *Combustion by the deflagrator is exceedingly vivid; the metallic leaves vanish in splendid corruscations; a platinum wire several feet in length, fixed between the poles while the metals are in the air, becomes red and white hot, and melts the instant they are immersed; the largest wire of this metal fixed in one pole and touched to charcoal in the other, melts like wax in a candle, and is dissipated in brilliant scin-*

tillations; a watch spring or a large steel knitting needle, fixed in the same manner and touched to the charcoal point, burns completely away with a torrent of light and sparks; a stream of mercury flowing from a funnel is deflagrated with brilliant light, and an iron wire is fused and welded to another under water.

3. *There is no perceptible impediment or loss in the flow of the galvanic current, from another room, through a circuit of 150 feet of apparatus and communicating leaden rods; the sparks may be taken at any intermediate points by connecting the two sides of the battery, and very beautiful combustions are produced by running metallic leaves or wires connected with one pole rapidly along the leaden rod which is the conductor to the other.*

4. *The shock from the deflagrator is, as I have thought, rather more severe than from an equal number of pairs of the common battery; probably this is on account of its being received when it is at a maximum.*

5. *All the effects of the deflagrator are easily renewed, from day to day with the same fluid, provided we add to it occasionally a little fresh acid; it exhibits a decided magnetic energy."*

The deflagrator was in fact a mobilized voltaic pile, and powerful deflagrators were in common use in America long before any apparatus of equal power was known in Europe.

Physicists and electro-chemists, in particular, appreciated the remarkable advance made by Hare in bringing to the scientific public his *calorimotor* and *deflagrator*. Those who were compelled to rely entirely on the voltaic pile must have felt that there were vastly greater things to be realized if the current could in some way be augmented. In private they had no doubt been seeking improvements. The renowned Faraday was of this group. After years of search he found a battery, but then he learned Hare had anticipated him. How



gracefully the great philosopher conducted himself in the consciousness of the superiority of his American colleague's contribution is patent from these lines:

“ Guided by these principles I was led to the construction of a voltaic trough. . . . On examining, however, what had been done before, I found that the new trough was in all essential respects the same as that invented and described by Robert Hare.”

And in another place, later perhaps, Faraday remarked that the deflagrator eminently associated the requisites of which he was in search, and alluded to many facts and arguments, tending to prove that it was the most perfect form of the apparatus, at that time, known.

Hare's views on heat, light and electricity were unique. He opposed the conjecture that heat may be motion. His ideas are clearly defined in the following letter to Silliman (1822):

“ Dear Sir:

In two memoirs published in the *Journal*, I have endeavored to shew that caloric and electricity, are collateral agents in galvanism, the ratio of the former to the latter, in quantity, being as the extent of the operating superficies to the number of pairs into which it may be divided. In those publications I assumed, that the causes of heat and electricity are material fluids. Although this view of the origin of calorific repulsion is taken by a great majority of chemists, it has been combated, both by Rumford, and Davy. With the utmost deference for the authority of these great men, especially the latter, I send the following remarks made in answer to his hypothetical views:

It is fully established in mechanics, that when a body in motion is blended with and thus made to communicate motion to another body, previously at rest, or moving slower, the velocity of the compound mass after the impact will be found,

by multiplying the weight of each body, by its respective velocity, and dividing the sum of the products, by the aggregate weight of both bodies. Of course it will be more than a mean or less than mean, accordingly as the quicker body was lighter or heavier than the other. Now according to Sir Humphry Davy, the particles of substances which are unequally heated are moving with unequal degrees of velocity: of course when they are reduced by contact to a common temperature, the heat, or what is the same (in his view), the velocity of the movements of their particles, ought to be found by multiplying the heat of each by its weight and dividing the sum of the product by the aggregate weight. Hence if equal weights of matter be mixed, the temperature ought to be a mean; and if equal bulks, it ought to be as much nearer the previous temperature of the heavier substance as the weight of the latter is greater; but the opposite is in most instances true. When equiponderant quantities of mercury and water are mixed at different temperatures, the result is such as might be expected from the mixture of the water, were it three times heavier; so much nearer to the previous heat of the water, is the consequent temperature. It may be said that this motion is not measurable upon mechanical principles. How then, I ask does it produce mechanical effects? These must be produced by the force of the vibrations, which are by the hypothesis mechanical; for whatever laws hold good in relation to moving matter in mass, must operate in regard to each particle of that matter; the effect of the former, can only be a multiple of that of the latter. Indeed one of Sir Humphry Davy's reasons for thinking heat to consist of corpuscular motions is that mechanical attrition generates it. Surely then a motion, produced by mechanical means, and which produces mechanical effects, may be estimated on mechanical principles.

In the case cited above, the power of reciprocal com-



munication of heat in two fluids, is shown to be consistent with the views of this ingenious theorist. If we compare the same power in solids, the result will be equally objectionable. Thus the heating power of glass being 443, that of an equal bulk of lead will be 487, though so many times heavier; and if equal weights be compared, the effect of the glass, will be four times greater than that of the lead. If it be said that the movements of the denser matter are made in less space, and therefore require less motion, I answer that if they be made with equal velocity, they must go through equal space and therefore, require less motion, I answer, that if they be made with equal velocity, they must go through equal space in the same time, their alternations being more frequent. And if they be not made with the same velocity, they could not communicate to matter of a lighter kind, a heat equally great; since, agreeably to experience, no superiority of weight will enable a body, acting directly on another, to produce in it a motion quicker than its own. Consistently with this doctrine, the particles of an aeriform fluid, when they oppose a mechanical resistance, do it by aid of a certain movement, which causes them effectively to occupy a greater space than when at rest. It is true, a body, by moving backwards and forwards, may keep off other bodies from the space in which it moves. Thus let a weight be partially counterbalanced by means of a scale beam, so that if left to itself it would descend gently. Place exactly under it another equally solid mass, on which the weight would fall unobstructed. If between the two bodies thus situated, a third be caused to undergo an alternate motion, it may keep the upper weight from descending, provided the force with which the latter descends, be no greater than that of the movement in the interposed mass, and the latter acts with celerity, that between each stroke the time be too small for the weight to move any sensible distance. Here then we have a case anal-

ogous to that supposed, in which the alternate movements or vibrations of matter enable it to preserve to itself a greater space in opposition to a force impressed; and it must be evident that lengthening or shortening the extent of the vibrations of the interposed body, provided they are made in the same time, will increase or diminish the space apparently occupied by it, as the volume of substances is affected by an increase or reduction of heat. It ought however to be recollected that in the case we have imagined, there is a constant expenditure of momentum to compensate for that generated in the weight by gravity, during each vibration. In the vibrations conceived to constitute heat, there is no generating power to make up for this loss. A body preserves the expansion communicated by heat in vacuo, where, insulated from all other matter, the only momentum, by which the vibrations of its particles can be supported, must have been received before its being thus situated. If we pour mercury into a glass tube shaped like a shepherd's crook, the hook being downwards, the fluid will be prevented from occupying that part of the tube where the air is in such position as not to escape. In this case, according to the hypothesis in question, the mercury is prevented from entering the space the air occupies, by a series of impalpable gyratory movements; so that the collision of the aerial particles against each other, causes each to occupy a larger share of space in the manner above illustrated by the descending weight and interposed body. The analogy will be greater, if we suppose a row of interposed bodies alternately striking against each other, and the descending weight; or we may imagine a vibration in all the particles of the interposed mass equal in aggregate extent and force to that of the whole, when performing a common movement. If the aggregate extent of the vibration of the particles very much exceed that which when performed in mass would be necessary to preserve a



certain space, it may be supposed productive of a substance like the air by which the mercury is resisted. But whence is the momentum adequate in such rare media to resist a pressure of a fluid so heavy as mercury, which in this case performs a part similar to that of the weight, cited for the purpose of illustration? If it be said that the mercury and glass being at the same temperature as the air, the particles of these substances vibrate in a manner to keep up the aerial pulsations; I ask, when the experiment is tried in an exhausted receiver, what is to supply momentum to the mercury and glass? There is no small difficulty in conceiving under the most favourable circumstances, that a species of motion, that exists according to the hypothesis as the cause of expansion in a heated solid, should cause a motion productive of fluidity or vaporization, as when by means of a hot iron, we convert ice into water, and water into vapour.

How inconceivable is it that the iron boiler of a steam engine should give to the particles of water, a motion so totally different from any it can itself possess, and at the same time capable of such wonderful effects as are produced by the agency of steam. Is it to be imagined that in particles whose weight does not exceed a few ounces, sufficient momentum can be accumulated to move as many tons? There appears to me another very serious obstacle to this explanation of the nature of heat. How are we to account for its relation in vacuo, which the distinguished advocate of the hypothesis has himself shown to ensue? There can be no motion without matter. To surmount this difficulty, he calls up a suggestion of Newton's, that the calorific vibrations of matter may send off radiant particles, which lose their own momentum in communicating vibrations to bodies remote from those, whence they emanate. Thus according to Sir Humphry, there is radiant matter producing heat, and radiant matter producing light. Now, the only serious objection

made by him to the doctrine which considers heat as material, will apply equally against the existence of material calorific emanations. That the cannon, heated by friction in the noted experiment of Rumford, would have radiated as well as if heated in any other way, there can, I think, be no doubt; and as well in vacuo, as the heat excited by Sir Humphry in a similar situation. That its emission in this way would have been as inexhaustible as by the conducting process cannot be questioned. Why then is it not as easy to have an inexhaustible supply of radiant matter, communicating the vibrations in which he represents heat to consist?

We see the same matter, at different times, rendered self attractive, or self repellent; now cohering in the solid form with great tenacity, and now flying apart with explosive violence in the state of vapour. Hence the existence, in nature, of two opposite kinds of reaction, between particles, is self evident. There can be no property without matter, in which it may be inherent. Nothing can have no property. The question then is, whether these opposite properties can belong to the same particles. Is it not evident, that the same particles cannot, at the same time, be self-repellent, and self-attractive? Suppose them to be so, one of the two properties must pre-dominate, and in that case we should not perceive the existence of the other. It would be useless, and the particles would in effect, possess the predominant property alone, whether attraction or repulsion. If the properties were equal in power, they would annihilate each other, and the matter would be, as if void of either property. There must, therefore, be a matter, in which the self-repellent power resides, as well as matter in which attraction resides.

There must also be as many kinds of matter, as there are kinds of repulsion, of which the affinities means of production, or laws of communication are different. Hence, I do firmly believe in the existence of material fluids, severally produc-



ing the phenomena of heat, light and electricity. Substances, endowed with attraction, make themselves known to us, by that species of this power, which we call gravitation, by which they are drawn towards the earth, and are therefore heavy and ponderable; by their resistance to our bodies, producing the sensation of feeling or touch; and by the vibrations or movements in other matter, affecting the ear with sounds, and the eye by a modified reflection of light. Where we perceive none of these usual concomitants of matter, we are prone to infer its absence. Hence ignorant people have no idea of air, except in the state of wind; and when even in a quiescent state designate it by this word. But that the principles, the existence of which has been demonstrated, should not be thus perceived, is far from being a reason for doubting their existence. A very slight attention to their qualities will make it evident, that they could not produce any of the effects, by which the existence of matter in its ordinary form is recognized. The self-repellent property renders it impossible that they should resist penetration; their deficiency of weight, renders their movements nugatory. When in combination, *they* are not perceived, but the *bodies* with which they combine; and it is only by the changes they produce in such bodies, or their effects upon our nerves, that they can be detected."

Silliman had been greatly interested in the fusion and volatilization of charcoal with the aid of the deflagrator, and wrote:

" My dear Sir,

" March 26, 1823.

In a former letter published in the *Journal*, Vol. V. p. 108, and in an additional notice, p. 361 same Vol., I gave an account of the fusion and volatilization of charcoal, by the use of your Galvanic Deflagrator. I have now to add, that the fusion of plumbago (black lead) was accomplished yesterday by the same instrument, and that I have, again, obtained

the same results today. For this purpose, from a piece of very fine and beautiful plumbago, from North Carolina, I sawed small parallelopipeds, about one eighth of an inch in diameter, and from three fourths of an inch to one inch and a quarter in length; these were sharpened at one end, and one of them was employed to point one pole of the deflagrator, while the other was terminated by prepared charcoal. Plumbago being, in its natural state, a conductor (although inferior to prepared charcoal), a spark was readily obtained, but, in no instance, of half the energy which belongs to the instrument when in full activity, for the zinc coils were very much corroded, and some of them had failed and dropped out; still the influence was readily conveyed, through the remaining coils. As my hopes of success, in the actual state of the instrument, were not very sanguine, I was the more gratified to find a decided result in the very first trial. To avoid repetitions I will generalize the results. The best were obtained, when the plumbago was connected with the copper, and prepared charcoal with the zinc pole. The spark was vivid, and globules of melted plumbago could be discerned, even in the midst of the ignition, *forming* and *formed* upon the edges of the focus of heat. In this region also, there was a bright scintillation, evidently owing to combustion, which went on where air had free access, but was prevented by the vapour of carbon, which occupied the highly luminous region of the focus, between the poles, and of the direct route between them. Just on and beyond the confines of the ignited portion of the plumbago, there was formed a belt of a reddish brown color, a quarter of an inch or more in diameter, which appeared to be owing to the iron, remaining from the combustion of the carbon of that part of the piece, and which, being now oxidized to a maximum, assumed the usual color of the peroxide of that metal.

In various trials, the globules were formed very abun-



dantly on the edge of the focus, and, in several instances, were studded around so thickly, as to resemble a string of beads, of which the largest were of the size of the smallest shot; others were merely visible to the naked eye; others still were microscopic. No globule ever appeared on the point of the plumbago, which had been in the focus of heat, but this point presented a hemispherical excavation, and the plumbago there had the appearance of black scoriæ or volcanic cinders. These were the general appearances at the copper pole occupied by the plumbago.

On the zinc pole, occupied by the prepared charcoal, there were very peculiar results. This pole was, in every instance, elongated towards the copper pole, and the black matter accumulated there, presented every appearance of fusion, not into globules, but into a fibrous and striated form, like the half flowing slag, found on the upper currents of lava. It was evidently transferred, in the state of vapor, from the plumbago of the other pole, and had been formed by the carbon taken from the hemispherical cavity. It was so different from the melted charcoal, described in my former communications, that its origin from the plumbago could admit of no reasonable doubt. I am now to state other appearances which have excited in my mind a very deep interest. On the end of the prepared charcoal, and occupying, frequently, an area of a quarter of an inch or more in diameter, were found numerous globules of perfectly melted matter, entirely spherical in their form, having a high vitreous lustre, and a great degree of beauty. Some of them, and generally they were those most remote from the focus, were of a jet black, like the most perfect obsidian; others were brown, yellow, and topaz colored; others still were greyish white, like pearl stones with the translucence and lustre of porcelain; and others still, limpid like flint glass, or, in some cases, like hyalite or precious opal, but without the iridescence

of the latter. Few of the globules upon the zinc pole were perfectly black, while very few of those on the copper pole were otherwise. In one instance, when I used some of the very pure English plumbago (sawed from a cabinet specimen, and believed to be from Borrowdale), white and transparent globules were formed on the copper side.

When the points were held *vertically, and the plumbago upper most*, no globules were formed on the latter, and they were unusually numerous, and almost all black, on the opposite pole. When the points were exchanged, plumbago being on the zinc, and charcoal on the copper end, very few globules were formed on the plumbago, and not one on the charcoal; this last was rapidly hollowed out into a hemispherical cavity, while the plumbago was as rapidly elongated by matter accumulating at its point, and which, when examined by the microscope, proved to be a concretion in the shape of a cauliflower—of volatilized and melted charcoal, having, in a high degree, all the characteristics which I formerly described as belonging to this substance. Indeed, I found by repetitions of the experiment, that this was the best mode of obtaining fine pieces of melted charcoal.

In some instances, I used points of plumbago on both poles, and always obtained melted globules on both; the results were however, not so distinct as when plumbago was on the copper and charcoal on the zinc pole; but the same elongation of the zinc and hollowing of the copper pole took place as before. I detached some of the globules, and partly bedding them in a handle of wood, tried their hardness and firmness; they bore strong pressure without breaking, and easily scratched, not only flint glass, but window glass, and even the hard green variety, which forms the aqua fortis bottles. The globules which had acquired this extraordinary hardness, were formed from plumbago which was so soft, that it was perfectly free from resistance when crushed be-



tween the thumb and finger, and covered their surfaces with a shining metallic looking coat. These globules sunk very rapidly in strong sulphuric acid—much more so than the melted charcoal, but not with much more rapidity than the plumbago itself, from which they had been formed.

The zinc of the deflagrator is now too far gone to enable me to prosecute this research any farther at present; as soon as the zinc coils can be renewed, I shall hope to resume them, and I entertain strong hopes, especially from the new improved and much enlarged deflagrator, which you are so kind as to lead me soon to expect from Philadelphia.

April 12: Having refitted the Deflagrator with new zinc coils, I have repeated the experiments related above, and have the satisfaction of stating that the results are fully confirmed and even in some respects extended. The Deflagrator now acts with great energy, and in consequence I have been enabled to obtain good results when using Plumbago on *both* poles. Parallelopipeds of that substance  $\frac{1}{5}$  of an inch in diameter and one inch or two inches long, being screwed into the vices connecting the poles, on being brought into contact, transmitted the fluid, with intense splendor, and became fully ignited for an inch on each side; on being withdrawn a little, the usual arch of flame was formed for half an inch or more. Indeed when the instrument is in an active state, the light emitted from the plumbago points, appears to be even more intense and rich than from charcoal; so that they may be used with advantage, in class experiments, where the principal object is to exhibit the brilliancy of the light.

On examining the pieces in this, and in numerous other cases, I found them beautifully studded with numerous globules of melted plumbago. They extended from within a quarter of an inch of the point, to the distance of  $\frac{1}{4}$  or  $\frac{1}{3}$  of an inch all around. They were larger than before and perfectly visible to the naked eye; they exhibited all the

colours before described, from perfect black, to pure white, including brown, amber, and topaz colours; among the white globules, some were perfectly limpid, and could not be distinguished by the eye from portions of diamond. In different repetitions of the experiment with the plumbago points, there were some varieties in the results. In one instance only, was there a globule formed on the point; it would seem as if the melted spheres of plumbago as soon as formed, rolled out of the current of flame, and congealed on the contiguous parts. In every instance, the plumbago on the copper side, was hollowed out, into a spherical cavity, and the corresponding piece on the zinc side, received an accumulation more or less considerable. In most instances and in all when the Deflagrator was very active, besides the globules of melted matter, a distinct tuft or projection was formed on the zinc pole, considerably resembling the melted charcoal, described in my former communications, but apparently denser and more compact; although resembling the melted charcoal, as one variety of volcanic slag resembles another, it could be easily distinguished by an eye familiarized to the appearances. In one experiment the cavity, and all the parts of the plumbago at the copper pole, were completely melted on the surface, and covered with a black enamel. The appearances were somewhat varied when specimens of plumbago from different localities were used. In some instances it burnt, and even deflagrated, being completely dissipated in brilliant scintillations; the substance was rapidly consumed and no fusion was obtained. This kind of effect occurred most distinctly when there was a plumbago piece on the copper side, and a piece of charcoal on the zinc side. I have already mentioned the curious result which is obtained when this arrangement is reversed, the charcoal on the copper, and the plumbago on the zinc side; this effect was now particularly distinct and remarkable, the charcoal on the copper side was rapidly vola-



tilized, a deep cavity was formed, and the charcoal taken from it, was instantly accumulated upon the plumbago point, forming a most beautiful protuberance, completely distinguishable from the plumbago, and presenting when viewed by the microscope, a congeries of aggregated spheres, with every mark of perfect fusion and with a perfect metallic lustre. I would again recommend this arrangement when the object is to attain fine pieces of melted charcoal.

Apr. 14: In repeating the experiments to-day, I have obtained even finer results than before. The spheres of melted plumbago were in some instance so thickly arranged as to resemble shot lying side by side; in one case they completely covered the plumbago, in the part contiguous to the point on the zinc side and were without exception white; like minute, delicate concretions of mammillary chalcedony; among a great number there was not one of a dark colour except that when detached by the knife they exhibited slight shades of brown at the place where they were united with the general mass of plumbago. They appeared to me to be formed by the condensation of a white vapour which in all the experiments, where an active power was employed, I had observed to be exhaled between the poles and partly to pass from the copper to the zinc pole, and partly to rise vertically in an abundant fume like that of the oxide proceeding from the combustion of various metals. I mentioned this circumstance in the report of my first experiments, but did not then make any trial to ascertain the nature of the substance. Although its abundance rendered the idea improbable, I thought it possible that it might contain alkali derived from the charcoal. It is easily condensed by inverting a glass over the fume as it rises, when it soon renders the glass opaque with a white lining. Although there was a distinct and peculiar odour in the fume, I found that the condensed matter was tasteless, and that it did not effervesce with acids,

or affect the test colours for alkalies. Besides as it is produced apparently in greater quantity, when both poles are terminated by plumbago, it seems possible that it is white volatilized carbon, giving origin, by its condensation, in a state of greater or less purity, to the grey, white, and perhaps to the limpid globules.

The Deflagrator having been refitted only at the moment when a part of this paper had already gone to press, and the remainder is called for, I am precluded by these circumstances from trying the decisive experiment of heating this white matter by means of the solar focus in a jar of pure oxygen gas, to ascertain whether it will produce carbonic acid gas.

This trial I have this morning made upon the coloured globules obtained in former experiments; they were easily detached from the plumbago by the slightest touch from the point of a knife, and when collected in a white porcelain dish, they rolled about like shot, when the vessel was turned one way and another. To detach any portions of unmelted plumbago which might adhere to them I carefully rubbed them between my thumb and finger in the palm of my hand. I then placed them upon a fragment of wedgewood ware, floated in a dish of mercury, and slid over them a small jar of very pure oxygen gas, whose entire freedom from carbonic acid, had been fully secured by washing it with a solution of caustic soda, and by subsequently testing it with recently prepared lime-water; the globules were now exposed to the solar focus from the lens. It was near noon, and the sky but very slightly dimmed by vapour; although they were in the focus for nearly half an hour, they did not melt, disappear, or alter their form; it appeared however, on examining the gas that they had given up part of their substance to the oxygen, for carbonic acid was formed which gave a decided precipitate with lime-water. Indeed when we consider that these globules had been formed in a heat vastly



more intense, than that of the solar focus, we could not reasonably expect to melt them in this manner, and they are of a character so highly vitreous, that they must necessarily waste away very slowly, even when assailed by oxygen gas. In a long continued experiment, it is presumable, that they would be eventually dissipated, leaving only a residuum of iron. That they contain iron is manifest, from their being attracted by the magnet, and their colour is evidently owing to this metal. Plumbago, in its natural state, is not magnetic, but it readily becomes so, by being strongly heated, although without fusion, and even the powder obtained from a black lead crucible after enduring a strong furnace heat, is magnetic. It would be interesting to know whether the limpid globules are also magnetic, but this trial I have not yet made.

I have already stated, that the white fume mentioned above, appears when points of charcoal are used. I have found that this matter collects in considerable quantities a little out of the focus of heat around the zinc pole, and occasionally exhibits the appearance of a frit of white enamel, or looks a little like pumice stone, only, it has the whiteness of porcelain, graduating however into light gray, and other shades, as it recedes from the intense heat. In a few instances I obtained upon the charcoal, when this substance terminated both poles, distinct, limpid spheres, and at other times they adhered to the frit like beads, on a string. Had we not been encouraged by the remarkable facts already stated, it would appear very extravagant to ask whether this white frit and these limpid spheres could arise from carbon, volatilized in a white state even charcoal itself, and condensed in a form analogous to the diamond. The rigorous and obvious experiments necessary to determine this question, it is not now practicable for me to make, and I must in the mean time admit the possibility that alkaline, and earthy impurities may have contributed to the result.

In one instance contiguous to, but a little aside from the charcoal points, I obtained isolated dark coloured globules of melted charcoal, analogous to those of plumbago.

The opinion which I formerly stated as to the passage of a current from the copper to the zinc pole of the deflagrator, is in my view, fully confirmed. Indeed, with the protection of green glasses, my eyes are sufficiently strong, to enable me to look steadily at the flame, during the whole of an experiment, and I can distinctly observe matter in different forms passing to the zinc pole, and collecting there, just as we see dust, or other small bodies driven along by a common wind; there is also an obvious tremor, produced in the copper pole, when the instrument is in vigorous action, and we can perceive an evident vibration produced, as if, by the impulse of an elastic fluid striking against the opposite pole.

If, however, the opinion which you formerly suggested to me, and which is countenanced by many facts, that the poles of the deflagrator are reversed, the copper being positive and the zinc negative be correct, the phenomena, as it regards the course of the current, will accord, perfectly well, with the received electrical hypothesis.

The number of unmelted substances being now reduced to two, namely, the anthracite, and the diamond, you will readily suppose I did not neglect to make trial of them, as however, the diamond is an absolute non-conductor and the anthracite very little better, I cannot say I had any serious hopes of success. I have made various attempts, which have failed, and after losing two diamonds, the fragments being thrown about with a strong decrepitation, I have desisted from the attempt, having, as I conceive, a more feasible project in view.

I trust you will not consider the details of the preceding pages, as being too minute, provided the subject appears to



you as interesting as it does to me. The fusion of charcoal and of plumbago, is sufficiently remarkable, but the evident approximation of the material of these bodies towards the condition of diamond, from which they differ so remarkably in their physical properties, affords if I mistake not, a striking confirmation of some of our leading chemical doctrines.

I remain as ever your faithful friend and servant,

B. SILLIMAN."

The failure of the deflagrator to act when connected up with ordinary voltaic apparatus disturbed Silliman very much,—so that in the following letter he discusses at length the relations existing between the Deflagrator and Calorimotor, and between these instruments and the common galvanic or voltaic batteries.

"Dear Sir:

"Yale College, April 4, 1823.

Through the medium of the Journal, I have already communicated to you and to the public, the singular fact, that your Deflagrator will not act with the common Galvanic Batteries, in whatever mode they may be connected, and that, although belonging to the same class of instruments and evolving the same imponderable agents, there still exists between them a total incompatibility. This incompatibility, it will be remembered, does not begin to be overcome, until the pairs of galvanic plates are reduced to twenty, in number, when the power of the Deflagrator begins to pass, and increases until one pair only is interposed, when it passes apparently without diminution.

I am induced again to call your attention to this fact, for the sake of connecting it, with some observations which I have recently made, upon the relations between the Calorimotor and Deflagrator, and between these instruments, and the common Galvanic Batteries, for it is only by varying our observations and experiments, that we can hope to arrive

at a just explanation, of the singular phenomena exhibited by these instruments.

1. I connected the zinc pole of the Calorimotor, with the copper pole of the troughs, and vice versa, and then dividing the troughs containing three hundred pairs of four inch plates, at another place, connected them at these new poles by points of well prepared charcoal; the sparks passed freely and vividly, nor did it, apparently make any difference, whether the plates of the Calorimotor, were immersed in the fluid, or not. I then disconnected the troughs from the Calorimotor, and connecting them together, received the spark, which was quite as vivid, as when the calorimotor formed a part of the series. I now immersed the calorimotor, and found that it acted by itself, with its appropriate energy, readily igniting iron, and displaying its usual magnetic activity.

2. The calorimotor and deflagrator were connected in such a manner, that the former was interposed between the two equal divisions of forty coils each, contained in the two troughs of the Deflagrator; in different trials, the connexion was varied, sometimes the zinc poles, and sometimes the copper poles of the two instruments, being connected, and at other times, the zinc of the one being joined to the copper of the other, and vice versa.

When the metals of both instruments were in the air, only a very feeble spark passed through the charcoal points connecting the proper poles of the Deflagrator. When the plates of the Calorimotor were immersed, those of the Deflagrator being in the air, the spark was not increased, but remained feeble as before. The coils of the Deflagrator being then immersed, the usual splendor of light, instantly burst from the charcoal points, and all the dazzling brightness and intense heat of the instrument were displayed, *but without any increase of power derived from the Calorimotor.* The plates of the Calorimotor were now raised from the



fluid, those of the Deflagrator remaining immersed, but the light and heat were equally brilliant as before. The Deflagrator and Calorimotor were now separated, and each produced its appropriate effects, in full energy.

3. The Calorimotor—the Deflagrator and the troughs containing the three hundred pairs of four inch plates, were now connected into one series, in such a manner that the Calorimotor was interposed between the two halves of the Deflagrator, the proper poles of the latter instrument were connected with the two divisions of the troughs; first, zinc, with copper, and copper with zinc, then the reverse, and the power was received at the proper poles of the troughs, charcoal points being used as before.

When the metals both of the Deflagrator and Calorimotor were in the air, a spark passed, such as corresponded with the power of the troughs only; when the Calorimotor was immersed, this power was neither increased nor diminished; but when the Deflagrator was immersed, its power flowed freely through the batteries, and was received apparently undiminished at the charcoal points, but did not appear to derive any increase from the troughs. This was the fact, whether the Calorimotor was, at the moment immersed, or not, but the lifting of the coils of the Deflagrator out of the fluid, immediately reduced the spark, to that which the troughs alone would afford.

The several instruments being now disjoined, each acted by itself, in its own appropriate character.

4. The original experiment of connecting the troughs with the Deflagrator only, was now again repeated, and with the same result as before; the power of both instruments was so destroyed, that only a very minute spark could be seen, and that with difficulty. From these experiments, and those formerly related, the following conclusions may be drawn:—

1. The galvanic troughs and the deflagrator paralyse each

other, and cannot be made by any means hitherto tried, to act in concert;

2. The Calorimotor does not impede the action of the troughs; it allows their energy to pass through itself, but contributes nothing to aid their power and cannot be made to project its own power through the troughs.

3. The same fact is true of the Calorimotor in relation to the Deflagrator; the powers of these instruments cannot be made to unite, only the Calorimotor allows a transit to the power of the Deflagrator; but the Deflagrator does not in its turn, transmit the power of the Calorimotor.

4. The Calorimotor, however, when connected, at once with the troughs and with the Deflagrator enables *them* so far to unite, that the deflagrator acts through the troughs, but without deriving any increase of power from them or from the Calorimotor; the Calorimotor then is an intermedium for the troughs and the deflagrator otherwise incompatible.

5. It is impossible as far as experiment has gone, to obtain any increase of power by combining the different kinds of voltaic apparatus, and indeed it may be doubted whether, when the power passes at all, through the instruments of different kinds, there is not always some loss, from the increased extent of connecting surface.

6. These various facts are probably all referable to the different powers, belonging to different proportions of the calorific, electrical, and luminous influence, excited by these different instruments, agreeably to the theory, which you have ingeniously proposed and ably defended; this view accords also with the known results of the combinations of ponderable elements, in the different proportions, as of nitrogen and oxygen, and of carbon and oxygen, and of carbon, hydrogen, and nitrogen.

7. We are thus sent back, to study our imponderable elements anew, and to learn, that the voltaic power is not



electricity alone, nor heat alone, nor light alone, but a compound of these three agents, variously proportioned in different cases, and in different modifications of apparatus. This, it appears, is also true, of the common mechanical and atmospheric electricity.

REMARK.

As the magnetic influence attends all the modifications of electricity, natural and artificial, and of the voltaic power, including your new instruments; and as it is exhibited also by the solar beam, we are left in doubt, whether to regard it as a mere appendage of these powers, or of some one or two of them, or as a distinct influence or energy, *incidentally* associated, with the calorific—calorific and electrical powers.

But, as the magnetic influence is marvellously more powerful, in the Calorimotor, than in the case of any voltaic, electrical or optical instrument, and as the Calorimotor evolves chiefly heat, and produces its magnetic effects *best* when it produces *no light* and *no perceptible electricity*, it would seem as if the magnetic influence were rather an attendant, on caloric, or at least in a greater degree, than on any other power.

It is extremely obvious, that, on all these subjects, we are still very humble learners; we may however, confidently hope, that out of these diversified results, and from others still to be obtained—some *grand simplification* will hereafter arise, which will reconcile all apparently discordant facts, and perhaps evince, that all the imponderable influences are merely modifications of one power—that they constitute the *atmosphere*, which connects physical existence with its author, and exhibit to us, in the natural world, the most immediate and wonderful efflux of his omnipotent energy.

Your friend and servant,

B. SILLIMAN."

On April 15, 1823, Silliman wrote Hare of new results which he had obtained by using the oxyhydrogen flame. His aim was to subject the diamond and anthracite to its intense heat. In the first experiments small diamonds were placed in a cavity in charcoal. The support, however, was so rapidly consumed, that the diamonds were speedily displaced by the gas current. He then took a piece of solid quick lime, made a chink in it and crowded the diamonds into it. The lime made an excellent support but "the effulgence of light was so dazzling, that, although through green glasses, I could steadily inspect the focus, it was impossible to distinguish the diamond, in the perfect solar brightness. This mode of conducting the experiment, proved, however, perfectly manageable, and a large dish, placed beneath, secured the diamonds from being lost, (an accident which I had more than once met with) when suddenly displaced by the current of gas; as however, the support was not combustible, it remained permanent, except that it was melted in the whole region of the flame, and covered with a perfect white enamel of vitreous lime. The experiments were frequently suspended to examine the effect on the diamonds. They were found to be rapidly consumed, wasting so fast, that it was necessary in order to examine them, to remove them from the heat, at very short intervals. They exhibited however, marks of *incipient fusion*. My experiments were performed upon small wrought diamonds, on which there were numerous polished facets, presenting extremely sharp, and well defined solid edges and angles. These edges and angles were always rounded and generally obliterated. The whole surface of the diamond lost its continuity, and its lustre was much impaired; it exhibited innumerable very minute indentations, and intermediate and corresponding salient points; the whole presenting the appearance of having been superficially softened, and indented by the current of gas, or perhaps of hav-



ing had its surface unequally removed, by the combustion. In various places, near the edges, the diamond was consumed, with deep indentations, and occasionally where a fragment had snapped off, by decrepitation, it disclosed a conchoidal fracture and a vitreous lustre. These results were nearly uniform, in various trials, and every thing seems to indicate that were the diamond a good conductor, it would be melted by the deflagrator, and were it combustible, a globule would be obtained by the compound blowpipe.

In one experiment, in which I used a support of plumbago, there were some interesting varieties in the phenomena. The plumbago being a conductor, the light did not accumulate as it did when the support was lime, but permitted me distinctly to see the diamond through the whole experiment. It was consumed with great rapidity; a delicate halo of bluish light, clearly distinguishable from the blowpipe flame, hovered over it; the surface appeared as if softened, numerous distinct but very minute scintillations were darted from it in every direction, and I could see the minute cavities and projections which I have mentioned, forming every instant. In this experiment I gave the diamond but one heat of about a minute, but on examining it with a magnifier, I was surprised to find, that only a very thin layer of the gem, not much thicker than writing paper remained, the rest having been burnt.

I subjected the anthracite of Wilkesbarre, Penn., to similar trials, and by heating it very gradually, its decrepitation was obviated. It was consumed, with almost as much rapidity, as the diamond; but exhibited, during the action of the heat, an evident appearance of being superficially softened; I could also distinctly see, in the midst of the intense glare of light, very minute globules forming upon the surface. . . . The remark already made, respecting the diamond, appears to be equally applicable to the anthracite, i. e. that its want of conducting power, is the reason why it is not

melted by the deflagrator, and its combustibility is the sole obstacle to its complete fusion by the compound blowpipe.

I next subjected a parallelopiped of plumbago to the compound flame. It was consumed with considerable rapidity, but presented at the same time, numerous globules of melted matter. . . .

In subsequent trials, upon pieces from various localities, foreign and domestic, (confined however to very pure specimens,) I obtained still more decided results; the white transparent globules became very numerous and as large as small shot; they scratched window glass—were tasteless—harsh when crushed between the teeth, and they were not magnetic. They very much resembled melted silix.

I find that the fusion of the plumbago by the compound blowpipe is by no means difficult, and the instrument being in good order, good results may be anticipated with certainty.

I would add, that for the *mere fusion of plumbago*, the blowpipe is much preferable to the deflagrator, but a variety of interesting phenomena in relation to both plumbago and charcoal are exhibited by the latter and not by the former. . . .

B. SILLIMAN."

In a postscript written three days later he continued, after commenting on his results with anthracite from various places:

"I have exposed a diamond this afternoon to the solar focus in a jar of pure oxygen gas, but observed no signs of fusion, nor indeed did I expect it, but I wished to compare this old experiment with those related above.

The diamond is now the only substance which has not been perfectly melted."

In this year (1824) Hare advised Silliman further as to improved deflagrators. Having found that the deflagrating power of a series of galvanic plates was surprisingly in-



creased by their simultaneous exposure to acid, various methods of accomplishing this suggested themselves. He informs him that in the apparatus he had sent him as all the coils were suspended from two beams they could be lowered into the troughs of acid. In another form which he had reported the troughs containing the acid were caused to rise which insured a simultaneous immersion of the plates; but a still better mode had suggested itself to him. This consisted in joining two troughs lengthwise, edge to edge, "so that when the sides of the one are vertical, those of the other must be horizontal;" so that by a partial revolution of the two troughs, thus united, upon pivots which support them at the ends, any fluid which may be in one trough, must flow into the other, and reversing the motion must flow back again. . . .

"The observations, which are the subject of this communication, combined with those which you have made, of the incapacity of the deflagrator, and Voltaic series in the usual form, to act, when in combination with each other; must justify us, in considering the former, as a galvanic instrument, having great and peculiar powers.

Since the above was written, I have tried my series of 300 pairs. The projectile power, and the shock, were proportionally great, but the deflagrating power was not increased in proportion. The light was so intense, that falling upon some adjacent buildings, it had the appearance of sunshine. Having had another series of 300 pairs made for Mr. Macnevin of New York, on trying it, I connected it with mine, both collaterally and consecutively, so as to make in the one case a series of six hundred,—in the other a series, half that in number, but equal in extent of surfaces. The shock of the two, consecutively, was apparently doubly as severe, as the shock produced by one; but the other phenomena seemed to me nearly equally brilliant, in either way.

The white globules which you mentioned, were formed copiously on the ignited plumbago, especially in vacuo. I have not had leisure to test them, being arduously occupied, in my course of lectures, and in some efforts to improve the means of experimental illustration."

As early as 1827 Olmsted made criticism upon the arguments which Hare had advanced "respecting the materiality of heat." Hare's reply presented nothing novel. It was throughout controversial, but a little later he resumed the subject, and as it shows how strongly he did appear in his discussions it may perhaps be well to reproduce his language in extenso. He was truly no mean antagonist. He thus begins:

"In the last number of the American Journal of Science, Professor Olmsted alleges that I have committed an oversight in making Davy's hypothesis "wear a much more mechanical aspect" than it did originally, and in "Applying to it principles which have no bearing on it whatever."

According to Johnson's Dictionary, mechanics is the geometry of motion, a science which shews the effect of powers, or moving forces, so as they are applied to engines, and "*demonstrates the laws of motion.*"

The phenomena of heat being by Sir H. Davy ascribed to motion, how can my arguments, shewing that they are not agreeable to the laws of motion, makes that hypothesis unduly "*wear a mechanical aspect,*" or subject it to an application of principles "*which have no bearing on it whatever?*"

In his first critique, the author alleged Davy's reasonings to be "*idle*" because they were "*mechanical.*"

A sufficient answer to this objection, was afforded in my essay in the following language:

"It may be said that this motion is not measurable upon mechanical principles. How then, I ask, does it produce mechanical effects? These must be produced by the force of



the vibrations, which are by the hypothesis mechanical: for whatever laws hold good in relation to moving matter in mass, must operate in regard to each particle of that matter. The effect of the former, can only be a multiple of that of the latter. Indeed one of Sir Humphry Davy's reasons for attributing heat to corpuscular vibration, is, that mechanical attrition generates it. Surely then a motion produced by mechanical means, and which produces mechanical effects, may be estimated on mechanical principles."

"In the hypothesis (says Professor Olmsted), the motions supposed, are those which occur between particles of matter, and at insensible distances. In the refutation, the principles applied are such as belong to those motions which occur between masses of matter, and at sensible distances."

The laws which regulate the production, or transfer, of motion, being established as respects any given mass, or quantity, can the division of it into two parts, ten parts, or a million parts, or into any possible number of parts, or particles, render those laws inapplicable? The same argument may be opposed to his distinction between the sensible and insensible distances, as if a law could cease to operate in consequence of the spaces being too small for our vision!!!

Since a whole can be no more than a multiple of its parts, a law cannot be true of motion, in any given distance, which does not hold good with respect to any part of that distance.

The minuteness of the distances within which movements can take place, in solids, is cited by me, as a potent objection to ascribing to intestine motion the expansive power imparted by them, when heated, to vaporizable substances, as in the case of water converted into steam by hot iron; but as such phenomena do result from intestine motion, and if the transfer of expansive power, be a transfer of such motion, however insensibly small may be the spaces in which it occurs, however minute the atoms concerned, how otherwise can they be

regulated, than by the same laws which are found to hold good in the case of larger spaces, and larger masses.

Professor Olmsted proceeds:

“The motions contemplated by the hypothesis, are either rotary, or vibratory; those supposed, in the refutation, are rectilinear, and in one continued direction; for to no other does the law of percussion adduced apply.”

As this allegation is unsupported by any proof, it can have but little weight. I will however throw my opinion into the opposite scale. I do assert that the law, which I have laid down, is universally applicable where motion is communicated, from one moving body, or set of bodies to another body, or set of bodies, whether the movements be vibratory, rotatory, or rectilinear.

If while two planets are revolving, or two pendulums vibrating, one overtake the other, will not the heavier be least altered from its previous motion? If two wheels, two globes, or two cylinders, while rapidly rotating, were to come into contact, would not the same law prevail?

“The refutation (says Professor Olmsted) supposes the particles to come into collision, each upon each; whereas the hypothesis does not warrant the supposition that any two particles ever strike against each other at all. For it is plain that the revolutions of particles round their own axes, do not bring them into collision with each other, nor do the vibrations of the particles make it necessary to suppose that they ever hit each other; for if there be space enough between the particles to permit them to vibrate at all, it is clear that they may vibrate without coming into collision.

“Finally, if they did impinge against one another, it must be remembered that the motion is backwards and forwards, and therefore this is not a case to which the law of percussion, as adduced by Dr. Hare applies.”

“I cannot but think therefore that Dr. Hare has refuted



a consequence, not of Sir Humphry Davy's but of his own creating."

It were obviously as absurd to allege, that particles cannot move without coming into collision, as to assert that the bow of a violin cannot move unless it rub against the strings. Yet as in the one case, friction is necessary to produce music, so in the other, collision is indispensable to keep the particles asunder. Would the diurnal movements of the planets prevent them from falling into the sun? Their annual motion has this effect, by generating a centrifugal force; but it cannot be imagined that in every mass, expanded by heat, the particles, by revolving about a common center of gravity, generate a centrifugal force which, counteracts cohesive attraction; and thus, enables them to exist at a greater distance from each other.

When by the affusion of hot water upon mercury, the temperature of the latter is raised, how can the velocity of the vibrations in which temperature consists, according to the hypothesis, be increased in the last mentioned liquid, without collision between the mercurial and aqueous atoms? While they remain asunder, the particles can have no influence upon each other, unless through the medium of some inherent property of attraction, or repulsion. On the former, motion is the opponent, of the latter the substitute, by the premises.

If motion be not productive of a collision among the particles, in what way can it enable them to sustain that remoteness, in their respective situations, which expansion requires? It cannot be supposed that they will become either reciprocally repulsive or less susceptible of cohesive attraction, merely in consequence of their undergoing a vibratory movement.

Professor Olmsted had evidently a very imperfect recollection of the design, or execution of my essay, when he wrote his critique; or he could not have denounced it as idly employing, in chemistry, those mechanical reasonings which it

was intended to explode. In the last number of the Journal, I devoted a page to the exposure of his error, in speaking of my essay, as intended to prove the materiality of heat, although described as remarks made in opposition to Davy's hypothesis. In the article now under consideration, he repeats this error in the following words.

"In the year 1822, Dr. Hare published an essay aiming to prove that caloric, or the cause of heat, is a material fluid."

I never wrote an essay of which this is a correct description. It did not appear to me expedient to recapitulate all the various well known arguments in favor of a material cause of calorific repulsion. To explain the phenomena of heat, but two hypotheses had been suggested, one ascribing them to caloric, the other to motion. The object of my essay was mainly to shew, that motion could not be the cause of heat, and I only incidentally introduced some direct arguments of a material cause.

I shall proceed to give other instances of the precipitancy of Professor Olmsted, in adopting the unfavorable impressions of my essay with which he occupies the pages of the American Journal of Science. The existence of repulsion and attraction as properties of matter, being referred to, as self-evident, and their co-existence as properties of the same particles, shewn to be inconceivable, I assumed that there must be a "matter in which repulsion resides," "as well as a matter in which attraction resides."

This induces Professor Olmsted to make the following inquiry:

"Does Dr. Hare maintain that the attraction which bodies exert, resides in a kind of matter extrinsic to the bodies themselves?"

It would be impossible, I think, to give a better answer to this query than is afforded by the following words of my neglected essay, words contained in the very next paragraph



below that which has given rise to Professor Olmsted's embarrassment.

"Substances endowed with attraction make themselves known to us by that species of this power which we call gravitation, by which they are drawn towards the earth and are therefore heavy or ponderable, by their resistance to our bodies, producing the sensation of feeling, or touch; and by the vibrations or movements which they excite in other matter, affecting the ear with sounds, and the eye by a modified reflection of light."

Will the Professor, after reading this sentence, require any further information respecting the kind of matter in which attraction resides, pursuant to my view of the subject? Independently of this sentence, *which I deem it unjustifiable in him to have neglected*, I do not know how he could take up the idea, that I considered the matter, in which attraction resides, as any other than that, usually recognized as matter, by people of common sense. Does my allegation that there must be as *many* kinds of matter as there are incompatible properties, convey the idea, that there must be *more* kinds of matter than there are of such properties?

Founding injudicious inferences with respect to my opinions upon errors, arising from his own inattention, the Professor proceeds:

"I have met with no late writer who has taken it for granted that there is matter in which attraction resides, distinct from the bodies themselves, which exert this influence on each other. But if Dr. Hare is not thus to be understood,—if he do not mean to assert such a doctrine, then why does he conceive it necessary to suppose a fluid upon which the phenomena of repulsion depend,—in which the self-repellent power resides, distinct from the bodies themselves, which exhibit such repulsion?"

I have said that the particles of ponderable matter ob-

viously possess the power of mutual attraction; they cannot then be endowed at the same time with reciprocal repulsion. But if they cannot be endowed with repulsion, why should they be endowed with attraction? says my antagonist.

If I were to allege the whiteness of a thing as a reason why it could not be black, would any person in his senses say, but if it cannot be black, how can it be white? Does the presence of attraction prove the absence of attraction, because it proves the absence of repulsion?

Since there is no permanent quality observed in the particles of ponderable matter, inconsistent with their exercising attraction, and as it would be unphilosophical to suppose more causes than are necessary to explain the phenomena, so it would be unreasonable to ascribe their attractive power to an extraneous principle. I allude to attraction of cohesion, or gravitation. That chemical affinity is much under the influence of the electric fluid, is now generally admitted. But to return to the critique.

*“Will Dr. Hare explain the fact that caloric sometimes increases the attraction of bodies for each other?”* “What would he say of the fact, that the attraction of two gases, is sometimes increased by heat?”

I will not undertake to explain that, which does not occur. When a mixture of hydrogen and oxygen gas is heated, it expands. So long as expansion continues it is obvious that caloric does not increase attraction. At the temperature of ignition the heterogeneous particles combine, and an explosion ensues.

Thus at the same moment that the simple atoms unite, the compound atoms, formed by their union, separate explosively. The elevation of temperature does not therefore increase attraction, it only favors the union of heterogeneous particles, by some unknown process. In a mixture of hydrogen and oxygen gas, the caloric with which they are severally combined, may attach itself to both poles of each



simple particle; after their union, to only one pole of each simple particle; and of course, to two poles of the compound particle forming water. Elevation of temperature may favor this change by its mysterious influence on the electric polarities of the particles; as in the case of the tourmaline:—or because the enlargement of the calorific atmospheres, renders the preservation of their independency more difficult.

That caloric is alternately an exciting cause of combination, and decomposition, we all know. Mercury is oxydised at one temperature, and revived at another. At one temperature hydrogen yields chlorine to silver, at another decomposes the chloride of that metal. At a low temperature, potassium absorbs oxygen more greedily than carbon, or iron, while the reverse is true, when these are heated to incandescence. I have long suspected that heat promotes and modifies chemical action, by influencing electrical polarities. The elements of water are severed by the voltaic poles. If in this case their polarity is influenced in one way, elevation of temperature, when it causes their reunion, must have an opposite effect, and of course must influence polarity.

I suppose in this case a change in the attractive power of the poles, of combining atoms, analogous to that which may be induced in iron bars, which attract or repel each other accordingly as the magnetism communicated to their poles, is alike or unlike.

Platina sponge, a cold metallic mass, is found to cause the union of the hydrogen and oxygen in a gaseous mixture: yet it is utterly inconceivable that the presence of inert particles, combining with neither of the elements of water, can cause an increase of attraction between them.

That the phenomena just alluded to, belong to a department of chemistry, with which we are but imperfectly acquainted, I admit; but on that very account inferences, founded on them, ought not to be allowed to invalidate the

demonstration, of which the existence of a material cause of heat is, upon other grounds, susceptible.

Professor Olmsted cannot discover that there is

“Any more difficulty in conceiving why a heated body should communicate its influence to another body without the aid of air, than *why the Sun should communicate his attractive influence to Saturn or Uranus without the aid of such a medium*”!!!

It would seem then that Professor Olmsted is of opinion, that the planets owe their power of attracting each other, and all the bodies on or near their surfaces to the Sun, as they owe their light; and that his removal from the system would simultaneously involve them in darkness, and destroy the reciprocal attraction between them, and their satellites. This is a glaring error. The reaction between the Sun and the planets, is reciprocal, arising from a quantity inseparable from either, and which admits of no increase, transfer, or diminution.

If the Sun did “*communicate his attractive influence*” to the other bodies in the solar system, I should be unable to say why he might not communicate any other property. The transmission of heat, *in vacuo*, is analogous to the radiation of light not the reciprocal influence of gravitation. If the illumination of Saturn or Uranus, could be explained without supposing the existence of a material fluid, I grant that the passage of heat *in vacuo* ought to admit of a similar explanation.

But as it is to me inconceivable, and contradictory to the obvious meaning of the word, to suppose the existence of a property without matter to which it may belong; so it appears impossible that there can be a transfer of a property, effected through a space otherwise void, without a transfer of matter.

The following paragraph was written in opposition to the *hypothesis of motion*, it is noticed by Professor Olmsted, as if intended directly to *support the materiality of heat*, as the reader will perceive by his remarks which I shall also quote.



"As in order for one body or set of bodies in motion to resist another body or set of bodies in the same state, the velocity must be as much greater, as the weight may be less, it is inconceivable that the particles of steam should by any force, arising from their motion, impart to the piston of a steam engine the wanted power; or that the particles of air should prevent a column of mercury, almost infinitely heavier, from entering any space in which they may be included by beating it out of the theatre of their vibratory, and rotatory movements.

"Has not Dr. Hare plainly fallen into a mistake here? It evidently is not heat which moves the piston of a steam-engine, but it is the elastic force of steam. But, it may be asked, is not that elasticity caused by heat? True; but the effect is not the same thing with the cause."

Was ever an inquiry more irrelevant? Where have I said that heat does move the piston of a steam-engine? In the paragraph above quoted which gives rise to the inquiry, I have only argued that motion produced among the aqueous particles, by the heated boiler, cannot move the piston. In order to shew that I have committed a mistake "*here*," it must be proved that *it is conceivable that the particles of steam should by a force arising from their motion, impart to the piston the wanted power*, or that particles of air, should, in like manner, "*support a column of mercury infinitely heavier*."

It evidently would be absurd to suppose that the piston of a steam engine could be propelled, by the direct influence of caloric, without the intermediate effect of the elasticity of vapor.

The author combats strange opinions, peculiar to his own imagination, as if I were answerable for them.

"It is difficult," says Professor Olmsted, "to see why heat should impart such a wonderful power to steam; nor does our supposing it to be a material *fluid* diminish this diffi-

culty." He might with equal propriety add, it is difficult to understand how light can impart to the objects around us, the wonderful property of conveying their images to the sensorium; nor does the idea of a material fluid, passing from them to the retina of the eye, diminish the difficulty.

It is difficult to understand why lead should be heavy; nor does the idea, that the earth attracts it, diminish the difficulty.

My mind is much less embarrassed by supposing a cause, where I observe an effect. Wonderful as it is, that the earth should by solar attraction be kept in its orbit, to me it is much less wonderful than if there were no sun to attract it; wonderful as it is that all the phenomena of vision should be due to the reflection, refraction, or polarization of a subtile matter emanated from every luminous point in the creation, the phenomena in question appear to me far less perplexing, than when I endeavor to dispense with the agency of a material cause. The opposite properties of the tenacity of ice, and the explosiveness of steam, however surprising, are less so when considered as belonging to different kinds of matter, than when I suppose them alternately assumed by the same particles, so as to cohere at one time, and at another fly apart, with violence, without any cause for the change.

It seems to me, that without the special interference of the Creator, the properties of any species of matter must always remain the same. Should any property appear to cease, or to be varied, there must be an accession, or an avolation of matter differently endowed, from that in which the change is observed.

"Has not Dr. Hare committed a mistake in understanding Sir Humphry Davy to assert that heat is motion; whereas, his doctrine is, that motion is the cause of heat."

The author forgets that the word heat is used to signify a cause as well as an effect; when I have spoken of motion



as substituted for heat, I meant that it was substituted for the cause of sensible heat. The phenomenon which we call sensible heat, is the effect of motion according to one hypothesis of caloric, or latent heat according to the other. It appears, therefore, that when correctly examined, the definition which I have given of Davy's hypothesis is the same as that which the author sanctions.

To conclude, I regret that instead of having only to encounter difficulties inherent in the subject, I should be obliged to occupy so many pages in refuting criticisms, respecting which, I can *sincerely say in the author's own language*, that they are "*idle,*" and have "*no bearing whatever*" upon the subject, which has called them forth."

Frequently the attention of Hare was directed to subjects having some connection with his favorite topic, electricity, and among these were the comments on inadequate protection afforded by lightning rods, so that it is not at all surprising to read (1828):

"This influence of the media, in which conductors terminate, has not been sufficiently insisted upon in treatises on electricity. I should not consider a metallic rod, terminating, without any enlargement of surface, in the water of the earth, as an adequate protection against lightning; but were such conductors to terminate in metallic sheets, buried in the earth or immersed in the sea, or *by a connexion duly made with the iron pipes, with which our city is watered, or the copper with which ships are generally sheathed*, I should have the most perfect confidence in their competency.

It is not only important that the points of contact, between the metallic mass, employed to afford lightning an adequate passage, and the earth or water, in which it terminates, should be so multiplied as to compensate for the inferior conducting power of the earth or water; but it is also

necessary that the conducting rod be as continuous as possible. When conductors are to be stationary, as when applied to buildings, they should consist of pieces screwed together, or preferably, joined by solder, as well as by screwing. When flexibility is requisite, the joints should be neatly made, like those of the irons in fall top carriages; and should be riveted so as to ensure a close contact at the junctures.

In all cases, the ordinary, but important precaution of having the rod to terminate above, in a fine clean point, should be attended to. Where platina tips cannot be had, multiplying the points by splitting the rod into a ramification of pointed wires, may compensate for the diminution of conducting power, arising from rust.

The efficacy of the point or points is, however, dependent on the continuity of the conductor of which I have already spoken; since it is well known, that if a pointed rod be cut into parts, so as to produce intervals, bounded by blunt terminations, its efficacy will not be much greater than if it had no point; because the fluid will, in that case, pass in sparks, instead of being transmitted in a current. It is on this account that I object to chains, or rods joined by loops or hooks and eyes."

Now and again he would burst forth in refutation of erroneous ideas contained in accepted texts. For example, after reading the following allegation in the American Edition of Turner's Chemistry:

"The electricity which is so freely and unceasingly evolved during the action of a good electrical machine, is derived from the great reservoir of electricity, the earth."

He wrote:

"I conceive that the earth has never, of *necessity*, any association with the phenomena of the electric machine; of which the power is evidently dependent on the efficacy of the



electric, in transferring the fluid from negative to the positive conductor. When the conductors are both insulated, by the revolution of the electric they are brought into states of excitement as opposite, as the power of the machine is at the time competent to produce. . . .

If the impression of the learned professor, were correct, how could a battery or a jar be charged, where both it, and the machine are insulated from the earth? Yet experience shows that it is under these circumstances that a charge is most easily imparted. When the conductors are in a state of excitement, and both insulated, the one will of course be as much below that of the surrounding neutral medium, and of the great reservoir, as the other is above that standard. When we connect either conductor with the earth, it returns of course to the neutral state of the earth; but the difference between the excitement of the conductors is sustained by the power of the machine to the same extent as before; hence the length and frequency of the sparks will not be found to be sensibly altered. It follows that when either of the conductors is made neutral by connexion with the earth, the other will have its excitement as much above or below neutrality, as the sum of the differences between each of the two conductors and the terrestrial neutrality when both are insulated. Thus supposing that when insulated, the one conductor is relatively to terrestrial electricity minus ten, and that the positive conductor is plus ten; when the negative conductor alone is uninsulated, the positive will be plus twenty, when the latter is alone uninsulated the former will be minus twenty.

It seems to be a common, though as I believe an erroneous idea, that a spark changes its character with the conductor from which it appears to be taken; so that when produced by presenting a body to the positive conductor, it is considered as positive, and as negative when produced with the negative conductor in like manner."

It is a pity that the entire correspondence between Hare and Silliman cannot be found, for there are interesting little items communicated from time to time. Thus he wrote:

"I have a magnet made essentially after the plan of Prof. Henry, excepting the use of paper and shell lac, in lieu of silk as an insulator, which method I devised and mentioned to you more than two years ago.

This magnet weighs seventeen pounds. It is surrounded by fourteen coils of copper wire, No. 15, each sixty feet in length. Its maximum of cohesive power is equal to seven hundred and eighty pounds.

I was curious to see if there would be any reaction between this magnet and the jet of igneous matter between the poles of a deflagrator, of seven hundred pairs of plates of four inches by three. The only remarkable result was, that the conducting power of the iron of the magnet was much reduced when subjected to the inductive influence of the coils.

This was demonstrated by attaching one pole of the series of seven hundred pairs to one leg of the magnet, while the other pole was made first to touch the end of the other leg, and then retracted so as to produce the vivid discharge of igneous matter, well known to ensue under such circumstances. The discharge being thus established, it was arrested as soon as a calorimotor was made to act upon the coils. The experiment was reiterated again, and again, with the same result.

About two years ago, I stated that taking the iron of an electro magnet into the circuit of a Calorimotor fifty times larger than that used for the coils, the attractive power, though enfeebled, was not destroyed. I have lately ascertained that a knitting needle may be magnetized and have its poles reversed while subjected to a direct current from the same large instrument, the inductive magnetic power being meanwhile due to a Calorimotor of not more than a fiftieth of the size."



It is impossible for any one who follows the life work of Robert Hare not to wonder about his surroundings while executing his great experimental problems. It will be recalled that in his early years the laboratory facilities (p. 12), were not very elaborate. At no time is there any indication from him or from others on the subject of laboratory appointments, so that when in 1831 he published an account of his laboratory and lecture room, illustrated by a plate drawing,<sup>2</sup> he conferred a real favor upon his colleagues not only of that period, but upon those who followed in the succeeding decades.

It is always a source of pleasure and delight to be acquainted, even slightly, with the side lights in the career of persons who have, in any wise, contributed to the advancement of the borders of human knowledge. The writer recalls with pleasure, as many another perhaps does, the thrill which came upon him while silently inspecting that chamber of the Deutsches Museum in Munich in which are assembled the various forms of apparatus showing the development of chemistry in Germany. To gaze upon an original Liebig condenser, or a combustion oven, carried him back to the days when the splendid foundations of the present organic chemistry were being quietly laid in the little German town on the Lahn. There rushed in upon his mind the magnificent problems which were there solved. In short, the objects collected in the Museum became a mighty inspiration. So too must the lecture hall and laboratory of Hare have been to all who were permitted to know them, for "no man in this country ever labored so much and so successfully for the improvement of practical chemistry as Hare."

The description of his "working place" reads almost like the account of Berzelius' laboratory as set forth in the inimitable word-picture drawn by the illustrious Wöhler. It reads:

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<sup>2</sup> See "American Jr. Science" (1st series), 19, 26: his Compendium (4th ed.), 1840; "Chemistry in America," D. Appleton and Company.

“ The hearth, behind the table, is thirty six feet wide, and twenty feet deep. On the left, which is to the south, is a scullery supplied with river water by a communication with the pipes proceeding from the public water works, and furnished with a sink and a boiler. Over the scullery is a small room of about twelve feet square, used as a study. In front of the scullery and study are glass cases for apparatus. On the right of the hearth two other similar cases, one above the other, may be observed. Behind the lower one of these is the forge room, about twelve feet square; and north of the forge room, are two fire proof rooms communicating with each other, eleven feet square each; the one for a lathe, the other for a carpenter's bench, and a vice bench. The two last mentioned rooms, are surmounted by groined arches, in order to render them secure against fire; and the whole suite of rooms which I have described, together with the hearth, are supported by seven arches of masonry, about twelve feet each in span. Over the forge room is a store room, and over the lathe and bench rooms, is one room of about twenty by twelve feet. In this room there is a fine lathe, and tools.

The space partially visible to the right, is divided by a floor into two apartments, lighted by four windows. The lower one is employed to hold galvanic apparatus, the upper one for shelves, and tables, for apparatus, and agents, not in daily use. In front of the floor just alluded to, is a gallery for visitors.

The canopy over the hearth is nearly covered with shelves for apparatus, which will bear exposure to air and dust, especially glass. In the center of the hearth there is a stack of brick work for a blast furnace, the blast being produced by means of a very large bellows situated under one of the arches supporting the hearth. The bellows are wrought by means of the lever represented in the engraving, and a rod descending from it through a circular opening in the masonry.



There are two other stacks of brick work on the hearth against the wall. In one there is a coal grate which heats a flat sand bath, in the other there is a similar grate for heating two circular sand baths, or an alembic. In this stack there is likewise a powerful air furnace. In both of the stacks last mentioned, there are evaporating ovens.

The laboratory is heated not only by one or both of the grates already mentioned, but also by stoves in the arches beneath the hearth, one of these is included in a chamber of brick work. The chamber receives a supply of fresh air through a flue terminating in an aperture in the external wall of the building, and the air after being heated passes into the laboratory at fifteen apertures, distributed over a space of thirty feet. Twelve of these apertures are in front of the table, being four inches square, covered by punched sheet iron. In the hearth there is one large aperture of about twelve by eighteen, covered by a cast iron plate full of holes, the rest are under the table. By these means the hot air is, at its entrance, so much diluted with the air of the room, that an unusually equable temperature is produced, there being rarely more than two degrees of Fahrenheit difference between the temperature in the upper and in the lower part of the lecture room. There are some smaller windows to the south, besides those represented in the engraving. One of these is in the upper story, from which the rays enter at the square aperture in the ceiling over the table on the right. Besides these, are the windows represented in the engraving back of the hearth, and four others in the apartments to the north of the gallery. All the windows have shutters, so constructed as to be closed and opened with facility. Those which belong to the principal windows are hung like sashes with weights, so that they ascend as soon as loosened, and when the light is again to be admitted, are easily pulled down by cords and fastened. In addition to the accommodation

already mentioned, there is a large irregular room under the floor of the lecture room on the eastern side. This is used as a place to stow a number of cumbrous and unsightly articles which are, nevertheless, of a nature to be very useful at times. Also for such purposes, and for containing fuel, there is a spacious cellar under the lecture room and laboratory."

Would it be too much for the reader to imagine that he and the writer, some time before 1847, concluded to visit this "old workshop" and with their own eyes behold the evidences of Hare's manual dexterity? Entering the lecture room and turning to the cases on the right the first stop would be at the electrometer with a single leaf, by which the electricity excited by the touch of heterogeneous metals is made very evident after a single contact (1824).

Nearby is the improved blowpipe using alcohol. In it "the inflammation is sustained by opposing jets of vapour, without a lamp." It will be recalled that as early as 1819 it occurred to Hare to make the flame of hydrogen gas or alcoholic vapour, more luminous by an admixture of oil of turpentine.

And there on the gallery, just over the lecture table, is the electrical plate machine designed by Hare. Its plate is four feet in diameter. He considered its mounting preferable to any with which he was acquainted. It was in connection with this machine that he discoursed on the causes of the diversity in the length of the sparks. He said that Thompson stated in his valuable work on heat and electricity that if a long spark be taken between two knobs, as when severally attached to the positive and negative conductors of the electrical machine; the portion of the spark near the positive knob exhibits all the characters of positive electricity, while the remaining portion proceeding from the other knob displays all the characters of negative electricity.

"Although the learned and ingenious author does not



state what differences there are between the different portions of the spark, and wherefore, if any exist; he can, without a *petitio principii*, assume that they are such as to justify his conclusion. He proceeds to allege that there can be no doubt that every spark consists of two electricities; which, issuing severally from their respective knobs, terminate their career by uniting at the non-luminous portion of the spark, which is at a distance from the negative knob, of about one-third of the interval. Upon these grounds he infers that the positive electricity occupies two-thirds of the length of the spark, the negative one-third.

I presume that, agreeably to the theory which supposes the existence of two fluids, when the equilibrium between oppositely excited surfaces is restored by a discharge, whether in the form of a spark or otherwise, there must be two jets or currents passing each other; the one conveying as much of the resinous as the other does of the vitreous electricity. Of course no part of a spark can be more negative than it is positive, nor more positive than it is negative. Upon this ground, a suggestion of the same author, that the diminution of light near the middle of the spark results from the combination of the different fluids at this point, appears to me injudicious, since there is as little ground for supposing the union of the fluids to take place there as elsewhere. But admitting that the union does take place as supposed, is this a reason for the observed diminution of light? If, when isolated, either fluid is capable of emitting a brilliant light, should not their co-operation increase the effect? If, after their union, they do not shine, it can only be in consequence of their abandoning, at that moment, all the light with which they were previously associated. It cannot be imagined that the light accompanying one should neutralize that accompanying the other.

In deflagrating, by voltaic electricity, a wire of uniform

thickness, equally refrigerated, the most intense evolution of heat and light is always midway.

In truth, the theory which the learned author sanctions, requires two postulates so irreconcilable, that unless one be kept out of view, the other cannot be sustained. It requires that the fluids should exercise an intense reciprocal attraction adequate to produce chemical affinity, and of course, enter into combination when they meet, and yet rush by each other with inconceivable velocity, not only through the air, but also through the restricted channel afforded by a small wire. If the fluids combine at a point intervening between the surfaces from which they proceed, what becomes of the compound which they form? Is it credible that such a compound would afford no indication of its existence? But, again, how are two surfaces, the one previously deprived of a large portion of the negative electricity naturally due to it, the other made as deficient of the positive fluid, to regain their natural state? By a combination midway, the resinous and vitreous surcharges might be disposed of, but whence could the vitreous and resinous deficiencies be supplied?

Dr. Thompson, in common with the great majority of modern chemists, ascribes chemical affinity to the attraction between the two electricities combined with ponderable particles. As the combinations between such particles take place only in definite proportions, would it not be consistent that the fluids which give rise to them, should combine agreeably to those laws? But if the electrical compound, formed of the vitreous and resinous electricities, be decomposable by induction, as the theory in question requires, its constituents must be capable of uniting in every proportion.

Agreeably to the late investigations of the celebrated Faraday, equal quantities of the electric fluid are evolved by analogous chemical changes, in equivalent weights of different ponderable bodies. It may therefore be inferred, that in en-



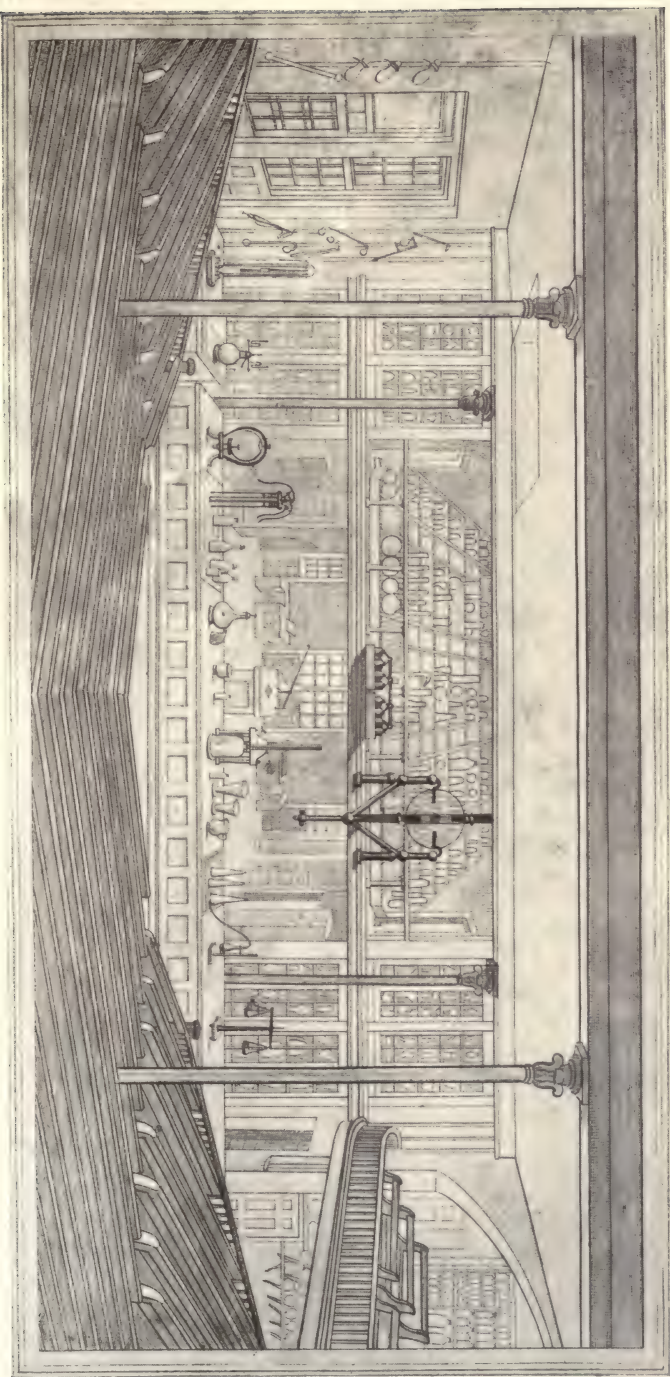
tering into combination the electric fluid is obedient to those laws of definite porportion which regulate other substances.”

In this connection hear Hare’s views on lightning rods:

“ In some of our American newspapers, a letter has been republished from the London Times, calculated, to lessen the confidence of the public in metallic conductors, as a mean of protection against lightning. The author of the letter appears to suppose, that metals are peculiarly attractive of electricity; and infers that, when a metallic rod is attached to a house, or ship, a discharge of electric fluid may be induced from a cloud, which, otherwise, would not have been sufficiently near to endanger the premises. Nothing in my opinion can be more erroneous than this notion. . . .

Nothing, to me, appears more unfounded than an idea, lately suggested, that the attraction between a ship, and a thunder cloud, can be increased, by the presence of a pointed metallic rod, surmounting the main-mast.

If houses, or vessels, have been struck with lightning, while provided with conductors, it is owing to the conductors being improperly constructed; or having no adequate connexion with the earth. . . . I object to chains, or rods jointed by loops, or hooks and eyes. The error of supposing that a metallic rod, must be more capable of attracting electricity injuriously, because of its known wonderful power in transmitting it, will be evident, when it is understood that the only difference between metals and other bodies, arises from the superior power of transmission. Hence, when by a defective communication with the earth or sea, the efficacy of the metal, as a conductor, is diminished, or destroyed, its influence over a charged cloud is proportionably lessened. It follows, therefore, that so far as it acts, its action must be beneficial, unless its lower termination should, by an inconceivable degree of ignorance or inattention, be so situated, as to render it more easy for the electrical fluid to leave the rod, and pass through



LECTURE ROOM OF ROBERT HARE  
Laboratory on the sides and in the rear





a portion of the house or vessel, than to proceed, by means of the rod, into the earth or sea.

Thus Richman was killed by a conductor which he employed to receive electricity from the clouds, and to convey it to an electrometer, necessarily insulated: under these circumstances, the head of the professor being about a foot from the conductor, he became a part of the channel of communication with the earth. Had the apparatus been surrounded by a cage of wire, and this duly connected with a metallic rod soldered to a sheet of metal buried in the earth, Richman might have made his observations with perfect safety.

I must premise, that the apparatus, by means of which the phenomena alluded to were produced, consisted of a wire a mile long, supported and insulated, upon very high poles."

And note those columns in the room. It will be recalled that Hare encircled them with seven hundred feet of copper wire—about the thickness of a knitting needle.

"At one end the wire was connected with one of his large calorimotors, while the other terminated in a cup of mercury, into which there dipped a wire from the other pole of the calorimotor. On bringing a magnetic needle near the middle of the circuit, it was powerfully affected and when the circuit was first interrupted, and then re-established by removing the wire from the cup, and introducing it again, the influence appeared to reach the needle so quickly as if the circuit had not exceeded seven inches in the meridian, while the circuit was interrupted, and the end of the wire being then returned into the mercury, the deviation of the needle, and the contact of the wire with the metal, appeared perfectly simultaneous.

A wire was made to circulate with great rapidity by means of two wheels about which it passed like a band. The wheels being metallic, and severally connected with the different poles of a calorimotor, it was found that the motion



neither accelerated nor retarded the galvanic influence—and it made no difference whether the needle was placed near the portion of the wire which moved from the positive pole to the negative, or the portion which moved in the opposite direction.

If a jet of mercury, in communication with one pole of a very large calorimotor, is made to fall on the poles of a horse-shoe magnet communicating with the other, the metallic stream will be curved outwards or inwards, accordingly as one or the other side of the magnet may be exposed to the jet—or as the pole communicating with the mercury may be positive or negative. When the jet of mercury is made to fall just within the interstice formed by a series of horse-shoe magnets, mounted together in the usual way, the stream will be bent in the direction of the interstice, and inwards or outwards, accordingly as the sides of the magnet, or the communication with the galvanic poles, may be exchanged. This result is analogous to those obtained by Messrs. Barlow and Marsh, with wires, or wheels.

It is well known that a galvanic pair, which will, on immersion in an acid, intensely ignite a wire, connecting the zinc and copper surfaces, will cease to do so after the acid has acted on the pair for some moments,—and that ignition cannot be reproduced by the same apparatus, without a temporary removal from the exciting fluid.

I have ascertained that this recovery of igniting power does not take place—if, during the removal from the acid, the galvanic surfaces be surrounded either by hydrogen gas, nitric oxide gas, or carbonic acid gas. When surrounded by chlorine, or by oxygen gas, the surfaces regain their igniting power, in nearly the same time as when exposed to the air.

The magnetic needle is, nevertheless, much more powerfully affected by the galvanic circuit, when the plates have been allowed repose, whether it take place in the air or in any of the gases above mentioned.

I have not yet had time, agreeably to my intention, to examine the effect of other gases, or of a vacuum."

And, it is pretty certain that, when discussing (1824) the question with his students as to the existence of *two* electrical fluids (Du Faye) or *one* (Franklin) he adduced before them numerous facts and arguments "in opposition to the doctrine of two fluids."

There, to the left, is one of those celebrated *deflagrators*, and we can almost hear Hare say, as we rest our eyes on this instrument:

"De Butts availed himself of that alternation of surfaces, that omission of insulation, which I first used in my *calorimotor* . . . indeed, he employs the same principle of simultaneous immersion originally used in my *deflator*! How can he claim anything novel?

"How can he speak of the *coils* as if that form of the galvanic battery had originated with Offerhaus and Pepys—whereas this was one of the forms first contemplated by me?"

Turning again, to the case on the right, are several *volumeters*—instruments by which to take volumes of gas, at one time, precisely equal to those taken at another time.

"There are two kinds of volumeters; one calculated to be introduced into a bell glass, over water or mercury; the other may be fitted through an orifice as is usual in the case of filling a common bottle over the pneumatic cistern."

Observe the *gasometer* at the end of the table. See how it is suspended from a beam? Well, Hare devised that as a substitute for the English Gasometer chain, more difficult to execute. And next to that is the *sliding rod gas measure* differing from the sliding rod eudiometers, in having a valve which is opened and shut by a spring and lever, acting upon a rod passing through a collar of leathers. By means of this



valve, any gas, drawn into the receiver, is included so as to be free from the possibility of loss, during its transfer from one vessel to another.

Immediately above on a shelf is what Hare termed a *Barometer Gage Eudiometer*. It is well known . . . that if a receiver communicate simultaneously with an air pump, and a barometer gage, the extent of the exhaustion will be indicated by the height of the mercury in the gage tube; so that if there be a scale of equal parts associated with the tube, the quantity of air taken from the receiver at any stage of the exhaustion, will be to the quantity held by it when full, as the number opposite the mercurial column, when the observation is made, to that to which it would rise, if the receiver were thoroughly exhausted. Hence, having exhausted the vessel, thoroughly, if the mercury stand at 450 degrees, by the gage, on allowing any gaseous fluid to enter till it sinks to 150 degrees, the quantity in the receiver will be 300 parts; and if of this, by explosion, or any other means, any number of parts be condensed, the mercury in the gage must rise that number of degrees.

Did you mark the smaller instrument by the side of the eudiometer? Hare designated it the *subsidiary eudiometer* to be used when the quantity of the gas was too small to be measured into the bell glass by a volumeter.

Directly above it is the *carbonicometer*—an apparatus, by which to withdraw a known portion of residual air from the barometer gage eudiometer in order to wash it with lime water.

“By agitating the globe, the carbonic acid will combine with the lime in the water. This effected, the residual gas may be allowed to re-enter the eudiometer, where the quantity of it may be measured, and consequently the extent of the absorption known.”

That large and elaborate piece of apparatus on the left side, near the left end of the table, is the *volumescope*. It

is striking in its appearance. It served a splendid purpose. It shows how intensely earnest Hare was to instil fundamental principles. With that contrivance he illustrated the fundamental basis of the theory of volumes. And among other uses he demonstrated with it "the ratio in which nitric oxide and the oxygen of atmospheric air are condensed by admixture." It was also applied in the analysis of carbonic oxide so as "to show that the result confirms the theory of volumes"; in the analysis of olefiant gas, and in the analysis of a mixture of carbonic oxide with one or more of the gaseous components of carbon with hydrogen, as well as in the analysis of a mixture of ethylene—carbon monoxide and either hydrogen or nitrogen or both of the latter. What a splendid gas analyst Hare was! He had considerable to say in his experimental writings of the use of nitric oxide in eudiometry. He also referred frequently to nitrous oxide and emphasized its preparation from ammonium nitrate.

Do not overlook those two massive pieces of apparatus with gas bags attached to their sides. They were intended to illustrate the combustion of "pulverized metals" and "metallic leaves" in chlorine; also for the abstraction of oxygen from the atmospheric air, "leaving the nitrogen so situated as to be easily drawn from the containing vessel."

The two "vases," on the top shelf, tightened by screws, were substitutes for Woulfe's bottles.

And, yonder, phials, and other glass vessels were used to illustrate the influence of compression on the capacity of "air for caloric and moisture." Hare declared that "the tendency in the atmosphere to cloudiness, at certain elevations, may be ascribed to the rarefaction which air invariably undergoes, in circulating from the earth's surface to such heights."

The other pieces of apparatus on the adjoining shelf were employed to illustrate the capacities for heat.



Now let us examine some of the cases in the balcony. The first thing to arrest attention is the *Litrameter* (litra, weight, and meter, measure). Hare contrived it to determine specific gravity. Its efficiency is due to the principle "that when columns of different liquids are elevated by the same pressure, their weights must be inversely as their gravities."

And those bladders, lying there, were used in this way: Knowing that the principal difficulty in weighing gases accurately is due to the small proportion which the weight of any gas can have, to that of any receiver, capable of sustaining the unbalanced atmospheric pressure, consequent to exhaustion Hare was led to another plan of manipulation. "The weight of a bladder is exactly the same, however large or small the quantity of atmospheric air, which it may include, provided the air which may be within it, be under no greater compression, than that without. Hence, if by means of a volumeter, we introduce a known quantity of any other gas, one hundred cubic inches for instance, whatever the bladder gains or loses in weight, will be the difference between the weight of the gas introduced, and that of a like volume of air. If the gas be lighter, we must deduct the weight necessary to restore the equilibrium from 30.5 grains. . . .

The comparative gravities of gases may be found by means of two bodies, counterpoised . . . by ascertaining the rarefaction or condensation of each gas, which would make the bodies equiponderate in it, as if it were atmospheric air."

Directly in the center of the long lecture table is the *hydro-pneumatic cistern* constructed on the principle of one contrived by Silliman and himself as early as 1803.

On examining it carefully its remarkable adaptability for pneumatic work becomes surprisingly evident. It indeed must have been a source of pleasure and comfort to the great experimenter.

Do you notice those coils of copper lying at the bottom

of the case? They seem to be almost endless. Their story, or at least the story of some of them, is briefly this:

Hare prepared a coil of copper wire, No. 26, nearly a mile in length, by means of which, and a strap of copper, three inches in width, and 196 feet in length, he had been enabled to repeat the experiment of Joseph Henry, for exciting a Faradian current. The wire was covered with cotton, and was coiled upon a wooden sieve hoop. Being suspended over a pulley, and counterbalanced by a weight over the strap, when this was placed in the circuit of a calorimotor, so that the circuit might be broken by drawing one of the electrodes over a rasp or ratchet wheel, communicating with the coil, shocks were felt, when the distance of several feet intervened, and they became intolerable when the coil and strap were nearly in contact. Having this coil at command, it occurred to Dr. Hare, to ascertain how far it would be competent to act as a multiplier. It seemed to be a problem which was yet to be solved, how far the extension of the length of the coils employed would affect their efficacy. He had not heard of any one in which resort had been had to an extension so great as a mile. Actuated by these considerations, he supported his coil in a vertical plane, and placed upon the lower and under surface of the hoop, the magnetic needle of an ordinary multiplier. A five cent piece, and a disk of zinc of the same size, being separated by a piece of moistened paper, when one of the ends was made to touch the silver disk and the other the zinc, the needle moved nearly a quadrant at every contact. When the disk was divided into four parts, every one of them was adequate to produce a movement in the needle, when the coil was made the medium of discharge. That such minute portions of metal should be capable of creating an electrical current in so long a coil, and sufficiently copious to influence a magnetic needle, would have appeared incredible to him, had it not been thus proved experimentally.



That extensive voltaic apparatus, standing there half under the table, is a galvanic deflagrator. In construction it is exactly like the one he made for the Lowell Institute of Boston. Somewhere he has said (in the American Philosophical Society Proceedings, I think) :

“ It consisted of four troughs, each containing 100 pairs within a space of about 30 inches in length. The pairs, severally, are of the Cruickshank pattern, and about  $6\frac{1}{2}$  inches square, independently of the grooves, so as to expose about 42 inches of zinc surface. Every fifth plate is cemented into its groove by a compound of rosin and suet. The plates, intermediate between those thus cemented are made to fit tightly into their grooves; but in consequence of a slight obliquity in their sides, can be extricated by the aid of forceps, so as to be cleansed, and when expedient, scraped. The cementing of each fifth plate tends to prevent any injurious retrocession of the voltaic fluid; and yet when the intermediate four plates are removed, an interstice is vacated, sufficiently large to allow the stationary metallic surfaces to be reached by a scraper. The plates are all amalgamated, which not only renders them less susceptible of wasteful reaction with acid, but more susceptible of being cleansed. A strip of wood, 13 inches wide and 2 inches deep, is bored by a centre bit, so as to have eight vertical and cylindrical holes, which are all supplied with mercury. By means of ropes of copper wire, these holes are made to communicate severally with the poles of each of the troughs, so that every one of these had its corresponding mercurial receptacle. Arches of twisted copper wire are provided of such various lengths, that the receptacles may be connected in such manner as to cause the associated troughs to act either as one series of 400 pairs each of 42 inches of zinc surface; as a series of 200 pairs each of 84 inches of zinc surface; or as a series of 100 pairs each of 168 inches of zinc surface. In the usual mode of constructing

the voltaic apparatus, the diversities of power that appertain to an apparatus in which the ratio of the size of the pairs to their number varies, as above described, can only be produced by changes in the arrangement, which are too inconvenient to be employed; but, according to the contrivance described, are attainable simply by shifting the connecting arches, so as to alter duly the mode in which the receptacles are connected with each other.

By means of this apparatus, the deflagration of metals, the arched flame between charcoal points, the fusion of platina by contact with the aqueous solution of chloride of calcium, the welding of iron wire to a rod of the same metal under water, were all accomplished with the most striking success.

In repeating Davy's experiment, in which the arched flame between charcoal points was subjected by the influence of a permanent magnet, the reaction between the voltaic and magnetic fluids was so violent, as to be productive of a noise like that of small bubbles of hydrogen inflamed in escaping from the generating liquid. This last mentioned experiment was performed by request of Prof. Henry who manipulated in the performance of it.

Hare stated that he had for many years endeavored to draw the attention of men of science to the fact, that if, when a fine and a coarse wire of platinum are made to form the electrodes or poles of a powerful voltaic series of not less than 300 pairs, the coarse wire, while forming the positive end or anode, be introduced into a concentrated solution of chloride of calcium, and the fine wire be made to touch the surface of the solution, fusion of the extremity into a globule will follow every contact. But when the polarity of the wires is reversed, the resulting ignition is comparatively feeble.

This experiment, Hare stated, was repeated to the satisfaction of Professors Silliman, Henry and James Rogers, all of whom were present at the trial of the apparatus.



When the finer wire was plunged about an inch below the surface of the solution, it became luminous throughout, emitting rays of a brilliant purple hue.

For the fusion of platina wire, in the experiment above described, it was found necessary to use the whole series consecutively as 400 pairs; showing, Hare remarked, that there are effects which require a great number of pairs. He had, in previous experiments, found that fresh phosphuret of calcium was a conductor for 350 pairs of  $7 \times 3$ , but not for 100 pairs  $7\frac{1}{2} \times 14$ .

The deflagration of an iron wire by contact with mercury took place with phenomena which were never before witnessed by any of the spectators. At first the mercury was deflagrated with an intense silvery white light, after which there arose a vertical shower of red sparks, caused by the combustion of the iron. Lastly, a globule having accumulated at the end of the wire after a momentary stoppage of the reaction, an explosion took place, by which fragments of the globule, together with portions of the mercury, were projected to a great distance.

“It would seem,” said Hare, “as if a globule of peroxide of iron, having formed at the end of the wire, caused a temporary arrestation of the voltaic current; but that the apparatus, gaining energy in consequence of a transient repose, was unable to break through the globule so as to disperse its particles with violence.”

It must not be forgotten that in 1826 Hare was selected, together with Professors Patterson and Keating, “to make choice of an hydrometer to be used in ascertaining the amount of the duties to be levied on spirits imported into the United States.” He found the instruments in use to be of English make or modifications of them: He finally decided in favor of the Dicus hydrometer, but recommends that special study should be made of this subject and that the government should

authorize the committee "to incur a reasonable expense, in the requisite investigations."

At the same time he informed the public that he had been examining the method of determining gravities, and expected to exhibit something altogether new. This was evidently the prelude to a communication made by him in the same year (1826) dealing exhaustively with the problem in an experimental fashion, and consequently there at the very top of that narrow case is the new instrument for this purpose—the "*chyometer*" (chuo, to pour, and meter, measure)—which is his sliding rod eudiometer arranged for use with liquids. In ascertaining the specific gravity of a solid (a mineral) the process differs from the usual procedure only, "in using measures of water, instead of the brass weights, ordinarily employed." The chyometer, in short, makes new weights out of water for each process. With its aid Hare demonstrated how the specific gravity of a mineral might be learned without calculation, and without degrees.

Do you observe on the bottom of the case those two eudiometers? They are the *sliding rod eudiometer*, the one to be used with nitric oxide, or with liquids absorbing oxygen; the other with explosive mixtures. In the contrivance for exploding the gases, as well as in the mode for measuring them—the wire is ignited by galvanism instead of the electric spark.

These forms he modified very much. For one thing he soldered the igniting wire into the summits of two brass wires which pass through the bottom of the socket parallel to the axis of the glass recipient, within which they are seen. For a while Hare puzzled over the unsatisfactoriness of results and the inconveniency of his eudiometer if used with mercury, so prepared a new form, provided with a water gauge which enabled the analyst to render the gases within *in equilibrio* with the air without.

The cocks, sockets, screws and sliding-rods of the *mercurial eudiometer* were made of cast steel.



The *barometer gauge eudiometer* lying by the side of the mercurial eudiometer is a much improved instrument on the original. And directly above these last objects is an "*improved cryophorus*"—which consisted of two flasks of which the necks had flanged orifices and so secured in a wooden frame that by the pressure of two screws and gum-elastic disks the orifices of a tube were made to form with them severally, air tight junctures . . . "midway between the latter a female screw was soldered to the tube for the insertion of a valve cock by means of which, and a flexible tube extending to an air pump, the flasks could be exhausted and then closed." "The intelligent chemist will perceive that this apparatus may be applied to the purpose of desiccation by placing the article to be dried in one receptacle, and quick lime, calcium chloride or concentrated sulphuric acid in the other." How like our very modern desiccators!

We must pause a moment at the next piece of apparatus. Hare termed it the *culinary paradox*. It is to show ebullition by means of cold.

The apparatus consists principally of a glass matrass, with a neck of about three feet in length, tapering to an orifice of about a quarter of an inch in diameter. The bulb is bulged inwards, in the part directly opposite the neck, so as to create a cavity capable of holding any matter which it may be desirable to have situated therein. In addition to the matrass, a receptacle, holding a few pounds of mercury, is requisite. The bulb of the matrass being rather less than half full of water, and this being heated to ebullition, the orifice should be closed by the finger, defended by a piece of gum-elastic, and depressed below the surface of the mercury. Under these circumstances, the mercury rises as the temperature of the water declines, indicating the consequent diminution of pressure within the bulb. Meanwhile, the decline of pressure lowering the boiling point of the water,

the ebullition continues till the mercury rises in the neck nearly to the height of the mercury in the barometer.

By introducing into the cup formed by the bulging of the bulb, cold water, alcohol, ether or ice, the refrigeration, the diminution of pressure, and the ebullition are all simultaneously accelerated, since these results are reciprocally dependent on each other.

The advantage of this apparatus and method of operating, lies first in the certainty and facility with which the apparatus is secured against the access of the atmosphere; and in the next place, in the index of the diminishing resistance, afforded by the rise of the mercurial column.

While resting a few minutes let me read to you what Hare wrote on the backwardness in the oxides of nitrogen to part with their oxygen to phosphorus.

“ This characteristic in the case of nitrous oxide, may be illustrated by means of an apparatus like that employed for the combustion of phosphorus in oxygen with a tall cylindrical receiver, and a tube descending through the neck, and along the axis of the receiver, terminating in a capillary orifice over the cup for holding the phosphorus. The upper end of the tube, outside the receiver, is furnished with a cock, to which a gum-elastic bag inflated with oxygen is attached.

Under these circumstances, the receiver having been exhausted and filled with nitrous oxide; phosphorus, previously placed within the cup, may be melted without taking fire. But as soon as the cock communicating with the bag of oxygen is opened, an intense combustion ensues; since the oxygen, emitted in a jet from the capillary orifice of the tube, reaching the melted phosphorus, excites it into an active combustion, which the nitrous oxide afterwards sustains with great energy.”

The *air-pump* was a very much used and favorite instrument with Hare. The one on the table is of new construction.



It may be used either as an air-pump or condenser or as both. The operator can exhaust, condense, or transfer a gas from one cavity to another, or even pass it through a liquid. Hare regarded it as superior to the elegant pump which served him for years, and gave preference to "the new instrument."

Next we observe the *Discharger for Deflagrating Wires*. This apparatus Hare used in lieu of Henley's universal discharger. It consists, as we see, of two brass plates, secured to the pedestal by a screw bolt which passes through a hole made in each, near one extremity: the plates are thus allowed a circular motion about the bolt, so as to be set in one straight line, or in any angle with each other. On one of the plates near the extremity, not secured by the bolt, a brass socket is soldered, into which a glass column is cemented, surmounted by a forceps. At the corresponding end of the other plate, there is a brass rod, perpendicular to the plate, and parallel to the glass column. This rod is also furnished with forceps. Between these forceps, supported and insulated by the glass column, a wire is stretched, which may be of various lengths, according to the angle which the plates make with each other. The pedestal is metallic, or it may have a metallic plate at bottom, in communication with the external coating of the battery. This being accomplished, it is only necessary to charge the battery, without subsequently breaking the communication between the inner coatings of the jars, and the prime conductor, by which the charge is conveyed. In that case, touching the conductor is equivalent to a contact with the inner coatings of the jars, so far as electrical results are concerned. Hence, by causing one of the knobs of the discharger with glass handles, to be in contact with the insulated forceps, and then approximating the other knob to the prime conductor, the charge of the battery will pass, as it cannot descend by the glass column, nor reach the operator through the glass handles.

We almost overlooked the *rotary multiplier* arranged so

nicely in that balcony case. Hare contrived it in 1836. It is a galvanometer. He said that it "had value as an addition to the amusing if not to the useful implements of science."

Even minor apparatus—such as ordinarily would not attract chemists, received Hare's attention. For, those *syphons* were constructed by him. In the one "a cork is perforated in two places parallel to the axis. Through one of the perforations, the longer leg of the syphon passes: into the other, one end of a small lead tube is inserted. In order to support this tube, it is wound about the syphon until it approaches the summit, where a portion of about three or four inches in length, is left free, so that advantage may be taken of its flexibility, to bend it into a situation convenient for applying the lips to the orifice. About the cork, the neck of a stout gum-elastic bag is tied air tight. The joinings of the tubes with the cork, must also be air tight. The lower half of the gum-elastic bag is removed, as represented.

In order to put this syphon into operation, a bottle must be used, having a neck and a mouth of such dimensions as to form an air tight juncture with the bag when pressed into it. This object being accomplished, the air must be inhaled from the bottle, until the diminution of pressure causes the liquid to come over, and fill the syphon. After this, on releasing the neck of the bottle, the current continues, as when established in any other way.

In the second one with the more complete construction are two metal tubes, passing through perforations made for them in a brass disk, turned quite true. Through one of these tubes, which is by much the larger, the syphon passes, and is cemented air tight. The brass disk is covered by a piece of gum-elastic, which may be obtained by dividing a bag of proper dimensions. The covering thus procured, is kept in its place by a brass band or clasp, made to embrace both it, and the circumference of the plate, and to fasten by means of a screw.



Before applying the caoutchouc, it was softened by soaking it in ether, and a hole, obviously necessary, was made in the centre, by a hollow punch.

There is no difference between operating with this syphon, and the other, excepting that the juncture of the syphon with the bottle, is effected by pressing the orifice of the latter against the disk covered with gum-elastic.

The large egg-shaped vessel with a wide and fairly long, stoppered neck with a cylindrical coil on the outer surface, standing in that far case is *DeLuc's Column* modified by Zamboni, and still further by Hare, who applied it as an *electrical discriminator*.

And there is the *galvanic machine* which Hare devised and employed in producing ignition in rock blasting. In it the calorimotor figures prominently. It was an exceedingly valuable instrument in its day. Hare thought his method of communicating ignition for rock blasting might "be applied as the means of exploding a mine."

Yonder is his improved *Galvanometer* which was quite unusual in size. Its needles were about 19 inches in length. And adjacent is the single gold leaf electroscope which manifests an astonishing sensitiveness to the smallest electrical force.

The next device is intended to be used in transferring a liquid from a carboy, or cask to bottles—it is especially useful in the case of sulphuric acid. In principle it is the syphon exhausted by a small pump. "This apparatus," said Hare, "may be employed to raise liquors into a bar room, from casks in a cellar, with this advantage over the pump now used for that purpose; that the liquid does not pass through the pump."

And you recall that the master was constantly endeavoring to improve chemical processes. For instance, you surely remember how he made *anhydrous prussic acid* by letting

hydrogen sulphide act upon mercuric cyanide and condensing the volatilized acid in a "refrigerated phial." And, in exploding a mixture of hydrogen and chlorine, the flask containing them was so placed "that a mirror receiving the solar rays directly, reflected them upon the flask."

Hare illustrated the decomposition and recomposition of water upon an extensive scale by the use of that large eudiometer, at the right end of the table, supplied with platinum "'electrodes' agreeably to the language of the celebrated Faraday." Is it not a striking piece of apparatus, calculated to produce a marked impression? It was, in short, what every teacher of the science does at present in his experimental course of lectures, but Hare operated on a grander scale.

You are not too weary to hear his account of freezing water by the aid of sulphuric acid—are you? Well, it was like this:

"It appeared to me that the failure arose from imperfection in the vacuum. An excellent pump, with perfectly air tight cocks, is indispensable; and not only must the pump be well made, it must likewise be in good order. Neither should the packing of the pistons, the valves, nor the cocks, allow of the slightest leakage. If a pump has been used previously for freezing, by the experiment of ether, it will not be competent for the experiment in question, unless it be taken apart and cleansed.

Cocks of the ordinary construction, are rarely if ever perfectly air tight, and their imperfection always increases with wear. Under these impressions, having cleansed my air pump, and put it into the best order possible, for the purpose of obviating leakage through the cocks associated with the instrument, I closed the hole in the centre of the air pump plate by a screw, and for a receiver made use of a bell glass with a perforated neck furnished with a brass cap and a female screw, by means of which one of my valve cocks was



attached. A communication between the bell, and the chambers of my pump, was established through the valve cock and a flexible lead pipe. In this way I succeeded in preserving the vacuum, longer than when the cocks of the air pump were employed in the process; and accomplished the congelation of water by means of the vacuum and sulphuric acid.

Latterly, I have used an apparatus in which a brass cover is made to close a large glass jar so as to be quite tight. In operating, the bottom of the jar was covered with sulphuric acid, and another jar with feet, also supplied with acid enough to make a stratum half an inch deep on the bottom, was introduced. The bottom of the vessel last mentioned, was, by means of the feet, kept at such a height above the surface of the acid in the outer jar, as not to touch it. Upon the surface of the glass vessel, a small thin sheet brass was placed, made concave in the middle, so as to hold a small quantity of water.

The brass cover was furnished with three valve cocks, one communicating with the air pump, another with a barometer gauge, and the third with a funnel supplied with water. Under these circumstances, having made a vacuum on a Saturday, I was enabled to freeze water situated on the brass, and to keep up the congelation till the Thursday following. As the water in the state of ice evaporates probably as fast as when liquid, during the night, the whole quantity frozen would have entirely disappeared, but for the assistance of a watchman whom I engaged to supply water at intervals. At a maximum I suppose the mass of ice was at times about two inches square, and from a quarter to a half an inch thick. The gradual introduction of the water, by aid of the funnel and valve cock, also of the pipe by which it was conducted to the cavity in the sheet brass, enabled me to accumulate a much larger mass than I could have produced otherwise. The brass band which embraces the inner jar near the brim, with the three straps proceeding from it, serves

to keep this jar in a proper position; that is in fact concentric with the outer jar."

Having seen some of his chemical and physical apparatus, may we not, with propriety, pay a little visit to the man himself? On tapping the door of the study gently there at once comes a kind invitation to enter. We are profoundly impressed by meeting, at the threshold, a figure of real grandeur with a remarkable head and features; the frame robust—powerful and ample in structure; but the genial welcome—the smile in the eye—encourage us, so we hasten to congratulate him upon the wonderful things we have seen, while he smilingly receives our words in silence, for it seems he never gave much thought to his marvellous inventive powers and was more apt to refer to some theoretical topic to which he had given consideration. While we talk he turns to a case from which he removes a small sealed tube and, breaking off the end, drops on a plate a brown powder which immediately inflames and we are informed that this is the new pyrophorus, obtained by exposing Prussian Blue to a high heat, in a tube sealed at both ends.

There was next exhibited to us a remarkably beautiful specimen of *potassium*, in globular form, and we heard how it had been prepared by modifying Brunner's process which consisted in subjecting to intense heat, in a luted iron mercury bottle, carbonized cream of tartar mixed with coarsely powdered charcoal. The potassium, as it distilled over, was caught in a copper vessel containing naphtha. "I substituted an iron tube which becomes finally full of the metal and a carbonaceous mass, which sublimes during the operation. The tube is then removed and the end nearest the bottle screwed into a tapering tube, while the other orifice is closed by a cap, into which it fastens by screwing. The tube is then placed vertically in a furnace, through the bottom of which the tapering tube extends so as to be out of the way of the



heat. Under the orifice of this tube, a vessel may be placed, containing some naphtha, to receive the potassium as it descends in globules, after fusion or condensation from the state of vapour. The last portions are not evolved before the fire in the furnace reaches a white heat. . . . By these means, I procured last winter, at one operation, more than six ounces of potassium."

It was in reference to this that the accompanying letter was written:

"My dear Silliman "Philad<sup>a</sup> 20<sup>th</sup> Jan'

I beg pardon for not replying sooner to your letter of the 10<sup>th</sup> inst. but trust you are not so wanting in faith as [to] doubt that I have had sufficient reason for the delay.

The bellows which I employ in making potassium are about of the largest size usually employ'd by blacksmiths. The tuyere is four inches square so that when using them for the ordinary forge fire we have to counterbalance the upper board instead of loading it as is common to do— When to be used to produce the greatest heat we remove the counterpoise & without loading can blow with ample force against a stratum of coal one foot in height—

I have a square hole in the hearth about ten inches each way on which I place a . . . grate— I have some bars put together with screws so as to be widen'd or narrow'd lengthen'd or shorten'd thus. These bars are used to bind together Sturbridge fire bricks, two of which are shortened at a convenient place for laying the gun barrel across.

This furnace is seated over the grate of its dimensions being made to correspond with the length of the containing tube so as to let it bear about  $\frac{1}{2}$  inch at each end on the bricks under it—There should be about 3 inches in each side of the tube between it & the brick work—

I have not used lute to the tube but mean to try one next time—I am told farinaceous substances as oil cake meal

kneaded with clay answer in very high heats. Plumbago has also been recommended. I should advise to use some thin coating which might vitrify—The Blacksmiths use loam—Perhaps Borax & lime might be good—A thick luting may impede the heat too much—I prefer card teeth as being of the purest iron & very much divided like hair in a mode singularly calculated for exposure.

I could send you some waste card teeth at 20 cents p pound.

The box of minerals is at M<sup>r</sup> Jordans. I have had no opportunity of sending it. I have received & approve of your last and will attend to its suggestions.

Your faithful

F<sup>d</sup> ROB<sup>t</sup> HARE "

" It might be possible to baste the iron with some vitrifiable matter & improve the process & thus save it from injury in a greater degree somewhat as cooks serve their roast meat."

In another place he tells that from 1818 he had pursued the method of keeping potassium in glass without naphtha. I copied his account from an early publication. It reads:

" I have been accustomed to seal a tube at one end, then to heat it at a convenient distance from the end, and reduce the diameter by drawing it down to about a quarter of an inch. Into the tube thus prepared, hydrogen is made to enter, so as to exclude the air. The potassium being then introduced, and the open end of the tube, closed by means of a spirit lamp, the metal may be fused, and with a little dexterity may be transferred in pure globules to that part of the cavity of the tube which is between the sealed end and the narrow part. This object being effected, the tube is divided at that part, and sealed by fusion.

In this case the potassium usually falls upon the glass and adheres to it, presenting a perfectly brilliant metallic coating,



and preserves this appearance without diminution for years.

It is however liable to inflammation from slight causes when kept without naphtha. I had an ounce of it in a small phial for eighteen months which took fire on my venturing to divide the phial by means of a file."

On a certain occasion he tried to free small globules of potassium from naphtha by heating them in a sealed tube, "properly recurved to act as a retort." After the metal had been removed he sought to examine "the caput mortuum left in the tube used as a retort." He struck it with a hammer and was "startled by a violent detonation." Berzelius thought these explosions due to moisture, but Hare experienced them when moisture could not have contributed to the result. His belief was that they arose from a "reaction of potassium, naphtha and flint glass."

I am quite sure that you will enjoy his story of how he filled tubes with potassium. He said:

"I have succeeded in filling glass tubes with potassium in the following manner. One end of a tube is luted to one of the orifices of a cock; to the other orifice, the neck of a gum elastic bag of a suitable size is attached. The open end of the tube is reduced in diameter by means of a flame excited by the blowpipe, so as to have an orifice about large enough to receive a knitting needle. The gum-elastic bag is filled with hydrogen, and the cock closed. Meanwhile the potassium is heated in naphtha, in a larger tube, till it lies at the bottom in a liquid state.

In the next place, the bag is grasped with one hand and subjected to pressure, at the same time introducing the small orifice of the tube into the naphtha, the cock is opened till the hydrogen begins to escape in bubbles. The escape of the bubbles is kept up to prevent the naphtha from entering the tube, and to evacuate the bag. Before this is quite accom-

plished, the orifice of the tube is to be approximated to the surface of the potassium as nearly as possible without entering it, and just as the last of the gas is expelled, is to be merged in the metal. The cock is at the same time to be closed, and the pressure of the hand on the bag discontinued. The cock being in the next place very cautiously opened, the elasticity of the bag counteracts the pressure of the atmosphere within the tube; and the liquid potassium is forced to rise into it. This effect may be controlled by the cock, which is to be closed when the column of the metal has attained a satisfactory height. After being removed, cooled and separated from the cock, the tube may be closed by a covering of sheet gum-elastic, such as is procured by the inflation of bags softened by ether. Any portion of the contents thus preserved may be extricated by cutting off and fracturing a portion of the tube, adequate to yield the requisite quantity.

In order to guard against accidents the apparatus was heated in this process by a bath of naphtha; in a bath of hot water. For the object last mentioned, the vessels ordinarily used for the solution of glue were employed, the naphtha being placed in the inner vessel usually occupied by the glue.

I have long been in the practice of filling tubes with phosphorus by a similar process."

It will be remembered that Hare synthesized ammonia in a most original way: it consisted in the union of two volumes of nitric oxide and five volumes of hydrogen on directing this mixture in the form of a jet upon gently heated platinum sponge. He evidently was the first person to use platinized asbestos for such contact work.

As we sit and gaze upon these almost numberless objects of Hare's originality and skill there come to mind other remarkable things which he achieved. It was he who suggested the expediency of using the "galvanic fluid" to fire gunpowder below the surface of water. On one occasion



before the members of the American Philosophical Society he referred to "the safety, certainty and facility" with which this might be done. And said he again referred to it "in consequence of the recent publication of analogous experiments by his friend, Professor Daniell, of King's College, London, who, in the case in point, no doubt as in that in which he had " 'reinvented' Dr. Hare's concentric blowpipe, was ignorant of the result previously obtained in this country. Prof. Daniell had, in blasting, used the highly ingenious apparatus known as 'Daniell's sustaining battery' the contrivance of which had done him great honour; but he conceived that however preferable might be a battery of that kind, in processes requiring a permanent current; for a transient energetic ignition, such as is most suitable for blasting, the caloric motors which he had contrived would be decidedly more efficacious."

It was before the same Society that he told how on exploding the elements of water in contact with certain gases or essential oils, "the aqueous elements, instead of condensing, combine with the hydrogen and carbon to yield a permanent gas."

In laboratories where frequent recourse is had to the use of dry hydrogen chloride as, for example, in the expulsion of molybdic acid from pure sodium molybdate, in the form of  $\text{MoO}(\text{OH})_2\text{Cl}_2$ , the gas is evolved by dropping crude muriatic acid from a funnel tube into a round-bottom flask containing concentrated sulphuric acid with application of a very gentle heat. The reverse may be used—sulphuric acid into commercial muriatic acid. This procedure Hare made known to the chemical world.

All of us are familiar with the "roseate tint" which may be imparted to light from illuminating gas by the flame surrounded by mica. This we owe to Hare's ingenuity. He said that "a thin sheet of mica, curved into a cylindrical form so as to enter a glass chimney, will retain the form thus im-

parted, in consequence of its elasticity and the confinement of the including glass. Thus employed, mica would correct the lurid influence of gas illumination, so much objected to by all who are desirous to appear "couleur de rose."

Very neat chimneys had been constructed, and maintained in the cylindrical form, by frames of tin plate, secured by rivets. Of course, the more delicate the frames, consistently with due firmness, the better. However costly at first, mica chimneys, he believed, would be cheaper in the long run, than those in common use.

When employed within a glass chimney, as he had described, the mica afforded the glass much protection against the flaming gas.

The mica, by which these results were obtained, when in thick plates, had a brownish red tinge, whether seen by reflected or transmitted light.

Further, he invented the valve-cock or gallows screw, by which "communicating cavities" in separate pieces of apparatus can be connected and made perfectly air-tight.

Hare further devised an apparatus for separating carbonic oxide from carbonic acid, by means of lime water. It stands just in front of the *litrameter*.

"Lime water being introduced in sufficient quantity, into an inverted large bell glass, another smaller bell glass is supported within it. Both of the bells have perforated necks. The inverted bell is furnished with a brass cap having a stuffing box attached to it, through which a tube of copper slides air-tight. About the lower end of this tube, the neck of the gum-elastic bag is tied. The neck of the other bell is furnished with a cap and cock, surmounted by a gallows screw, by means of which a lead pipe with a brass knob at the end suitably perforated, may be fastened to it, or removed at any moment. Suppose this pipe, by aid of another brass knob at the other extremity, to be attached to



the perforated neck of a very tall bell glass filled with water upon the shelf of a pneumatic cistern; on opening a communication between the bells, the water will subside in the tall bell glass, over the cistern, and the air of the bell glass being drawn into it, the lime water will rise into and occupy the whole of the space within the latter. As soon as this is effected, the cocks must be closed and the tall bell glass replaced by a small one filled with water, and furnished with a gallows screw and cock. This bell being attached to the knob of the lead pipe to which the tall bell had been fastened before, the apparatus is ready for use. Hare said, "I have employed it in the new process for obtaining carbonic oxide from oxalic acid, by distillation with sulphuric acid in a glass retort. The gaseous product consists of equal volumes of oxide and carbonic acid, which, being received in a bell glass communicating as above described by a pipe with the bell glass, may be transferred into the latter, through the pipe, by opening the cocks. As the gaseous mixture enters the smaller bell, the lime water subsides. As soon as a sufficient quantity of the gas has entered, the gaseous mixture may, by means of the gum-elastic bag and the hand, be subjected to repeated jets of lime water, and thus depurated of all the carbonic acid. By raising the water in the outer bell the purified carbonic oxide may be propelled, through the cock and lead pipe, into any vessel to which it may be desirable to have it transferred and measured."

Hare also devised a *water-bath*, with funnel, for use in the filtration of hot, saturated solutions, so as to prevent too rapid crystallization. Modern forms of this apparatus exist.

And then we next saw "the twenty-three ounces of platinum which on a certain occasion he had fused with the oxy-hydrogen flame," as well as a specimen of pure platinum freed from iridium by the process of Berzelius; and the iridium and rhodium which had been fused. These latter metals

“became more fusible by continued and repeated fusion . . . both appeared to evolve some volatile matter, and did not become completely solid until after being repeatedly fused.” He also succeeded in fusing iridium osmiuret. In determining the specific gravity of iridium he found it to be 21.80—higher therefore than that of platinum. From this he remarked:

“An important inference from these results was, that as iridium is the only impurity in standard platinum, a high specific gravity indicates neither a superior degree of purity nor malleability.

A piece of standard malleable platinum, of a very fine white colour, presented to Dr. Hare by his excellency, Count Cancrine, the Russian minister of finance, as of the best quality of Russian platinum, proved, according to Eckfeldt, to have a specific gravity of 21.31; when a specimen, purified from iridium agreeably to the instruction of Berzelius, and which had been found pre-eminently susceptible of being beaten into leaf, weighed only 21.16.

On its first fusion, Hare found the specific gravity of rhodium to be 11; precisely what, on examining his books, it was ascertained to have been made by Wollaston. But after it had crystallized superficially, as above described, it was by a magnifier discovered to be minutely porous under the facets. In this state its specific gravity was found by Eckfeldt to be 10.8.

Observe that bottle labeled *fulminating silver*. Hare was accidentally badly injured by an explosion of this substance. His friend Silliman thereupon wrote and published the following:

“Many persons have sustained injuries, more or less severe, from fulminating silver, and much anxiety was felt for the safety of Dr. Hare, who met with a dangerous accident, of this kind, early in February (1832).

We learn from him that the quantity which exploded



was such, as in its light feathery state, nearly filled an ounce bottle. It had been dried on a filter, but, in three trials, failed to explode by percussion. By a subsequent exposure in the evaporating oven, it was rendered unusually explosive. Hence as Dr. Hare was in the act of pouring out a small portion, upon the face of a hammer, the whole exploded, without any obvious cause, unless as he suggests, it was a slight pressure, which might possibly have been created upon a particle of the powder, between the neck of the bottle and the hammer. By the explosion, the bones of all the fingers of the right hand, except the little finger, were more or less broken; part of the flesh of the terminating joint of the thumb was torn off together with the nail, and the latter was found upon the laboratory floor; the corresponding finger was much injured, and the palm bruised and lacerated.

His faithful and experienced assistant, George Workman, was holding the hammer at the moment of the explosion, and was consequently wounded in the face and eyes; into one of the latter, a spicula of glass was driven, which was not removed without skillful surgical aid; he recovered however, in a fortnight. A pupil was wounded slightly in the face, and Dr. Hare himself had a small fragment of glass removed from one of his eyes. Among his late and present colleagues, are some of the most skillful of surgeons, who, with his pupils, were immediately present to afford every necessary aid. It appears, that he had been accustomed, for six years, to pour from the same vial, such portions of fulminating silver as he needed for his experiments, and had never met with any accident. He had, in the present instance, prepared an unusual quantity with reference to some analytical experiments which he had proposed to perform by igniting the powder in a receiver of known capacity, by means of a wire galvanized *in vacuo*; the quantity of gaseous matter was to be ascertained by a gage, and the kinds by accurate eudiometrical analysis.

Happily, no tetanic symptoms followed, and although the patient suffered intensely, he has been mercifully spared to his family and friends and the world. Excepting some rigidity and tenderness in the renovated muscles, he is now recovered."

Another vial is marked *sugar* from the *sweet potato*, of which Hare said:

"Dr. Tidyman of South Carolina, lately supplied me with some *sweet potatoes*, of a kind in which sweet matter is peculiarly abundant, and requested that I would ascertain if there were any sugar in them. Having pared, and by means of the instrument used for slicing cabbages or cucumbers, reduced them to very thin slices; about a pound was boiled in alcohol of the specific gravity of 0.845, which appeared to extract all the sweetness, yet on cooling yielded no crystals of sugar. The solution being subjected to distillation, till the alcohol was removed, an uncrystallizable syrup remained. In like manner, when aqueous infusions of the potatoes were concentrated, by boiling or evaporation, the residual syrup was uncrystallizable. It appears therefore that the sweet matter of this vegetable is analogous to molasses, or the *sacchrum* to malt. Its resemblance to the latter was so remarkable, that I was led to boil a wort, made from the potatoes, of proper spissitude, say s. g. 1060, with due quantity of hops, about two hours.

It was then cooled to about sixty-five degrees, and yeast was added. As far as I could judge, the phenomena of the fermentation, and the resulting liquor, were precisely the same as if malt had been used. The wort was kept in a warm place until the temperature 85 F. and the fall of the heat showed the attenuation to be sufficient. Yeast subsequently rose, which was removed by a spoon. By refrigeration a further quantity of yeast precipitated, from which the liquor being decanted became tolerably fine, for new beer, and, in flavour, exactly like ale made from malt.



I believe it possible to make good liquor from malt in this country, as in England, but that in our climate much more vigilance is required to have it invariably good, principally because the great and sudden changes of temperature, render malting much more precarious. Should the *saccharum* of the sweet potato prove to be a competent substitute for that of germinated grain, the quality will probably be less variable, since its development requires but little skill and vigilance.

Besides, as it exists naturally in the plant, it may be had where it would be almost impossible to make, or procure malt. Hops, the other material for beer, require only picking and drying to perfect them for use.

They are indigenous in the United States, and, no doubt, may be raised in any part of our territory.

I have dried, in my evaporating oven, some of the sweet potatoes in slices. It seems to me that in this state they will keep a long while, and may be useful in making leaven for bread. They may take the place of the malt necessary in a certain proportion, to render distillers' wash fermentable. The yeast yielded by the potato beer appeared in odour and flavour to resemble that from malt beer surprisingly, and the quantity, in proportion, was as great. In raising bread, it was found equally efficacious.

I propose the word *suavin*, from the Latin *suavis*, sweet, to distinguish the syrup of the sweet potato. The same word might, perhaps, be advantageously applied as a generic appellation to molasses, and the uncrystallizable sugar of grapes, of honey, and of malt.

Crystallizable sugar might be termed *saccharin*, since the terminating of *saccharum* is appropriated in chemistry to metals."

In the little jar at the side of the vial containing fulminating silver is a specimen of *boron*. It was made (1833)

by the interaction of potassium and vitrified boracic acid *in vacuo*. In its preparation Hare made a circular brass plate, like the plate of an air-pump "so as to produce with any suitable receivers properly ground, an air-tight juncture." It was supported on the upper end of a hollow brass cylinder, with the bore of which it had a corresponding aperture. The brass cylinder was about three inches in diameter, and six inches in height, being inserted at its lower end into a block of wood as a basis. This cylinder received below, a screw, which supported a copper tube, of about two inches in diameter, so as to have its axis concentric with that of the cylinder, and to extend about four inches above the plate. The copper tube, thus supported, was closed at the upper termination by a cup of copper, of a shape nearly hemispherical, and soldered at the upper edge, to the edge of the tube; so that the whole of the cavity of the cup, was within that of the tube. Hence the bottom of the cup was accessible to any body, not larger than the bore of the tube, without any communication arising between the cavity of the tube, and that of any receiver placed upon the plate, over the cup and tube.

Into the side of the cylinder, supporting the plate, a valve cock was screwed, by means of which, and a flexible leaden tube, a communication with an air-pump was opened, or discontinued, at pleasure.

The cup being first covered with a portion of the vitrified boracic acid, as anhydrous as possible, and finely pulverized, the potassium was introduced, and afterwards covered with a further portion of the same acid, two parts of the potassium being used for one of the acid. A large glass receiver was then placed on the plate, secured by rods concentric with the tube and cup; from the heat of which the glass was to be protected by a bright cylinder of sheet brass, placed around it so as to be concentric with the receiver and tube.

The apparatus being so prepared, and the receiver ex-



hausted of air by means of the air-pump, an incandescent iron was introduced through the bore of the tube, so as to touch the bottom of the copper cup. In a short time a reaction commenced, which, aiding the influence of the hot iron, rendered the cup and its contents red hot. A deep red flame appears throughout the mass, after which the reaction lessens, and the heat declines.

When the cup has become cold, the air is admitted into the receiver, and the contents are washed with water. If any of the acid has escaped decomposition, it may be removed by boiling the mass with a solution of potash or soda. After this treatment and due desiccation a powder will remain, having the characteristic color and properties of boron.

The next bottle contains *silicon*. It will be remembered that Hare had isolated this element and also boron from their gaseous fluorides with the aid of potassium and the calorimotor. This happened in 1833. Some time after he resorted to a much simpler process:

“A bell glass, over mercury, was filled with fluosilicic acid, and by means of a bent wire, a cage of wire gauze, containing a suitable quantity of potassium, was introduced through the mercury into the cavity of the bell, and supported in a position nearly in the centre of it. A knob of iron was made at the end of the rod, so recurved as to reach the cage with ease. The knob, having been heated nearly white hot, was passed through the mercury, so as to touch the cage, and cause the combustion of the potassium and evolution of the silicon. Of this, much remains attached to the cage, in combination with the fluoride of potassium, from which the silicon may be separated by washing in cold water and digestion in nitric acid.

“The silicon thus obtained does not appear to be acted on either by sulphuric, nitric, fluoric, or muriatic acids; nor when exposed to nitrate of potash liquefied by heat. It seems

to be soluble for the most part in a mixture of nitric and fluoric acid, which by analogy we may call nitro-fluoric acid; but after exposure for eighteen hours to this solvent, a small proportion of a black matter remained undissolved. This is, in all probability, carbon derived from the potassium, which, according to Berzelius, when obtained by Brunner's process, is liable to be combined with carbon. The solution of nitro-fluoric acid, decanted from the residual black powder into a solution of pearlash, gave a copious, white, gelatinous precipitate like silex, which, when thrown into a large quantity of water, subsided undissolved. When on subjecting the silicon to red hot nitrate of potash, anhydrous carbonate of the same alkali was added, so as to co-operate with the nitre, an explosive effervescence took place, all the silicon disappeared, and a compound resembling the silicate of potash was produced. This anomalous reaction may be considered as characteristic of silicon.

The impression that the black matter insoluble in the nitro-fluoric acid, was carbon, is confirmed by the fact, that after the silicon had been digested for some hours in strong nitric acid, and finally boiled in it to dryness, it dissolved in a nitro-fluoric acid without any such residuum."

In the adjoining bottle there is a resin. Hare called it *sassarubrin* and wrote the same on the label of the containing vessel. This resin was obtained by the interaction of sulphuric acid and oil of sassafras. When the resin is dropped into concentrated sulphuric acid the latter acquires a crimson colour. Hare expressed the thought that a new series of resins might be evolved from the essential oils, by contact with sulphuric acid (1837).

One cannot but wonder whether the rich black ink, seen on all the labels, was made by Hare "by letting an infusion of galls stand over finery cinder till it was saturated?" He represented the product as most satisfactory, in all respects—"in which there is no free acid."



Here is a specimen of a *fulminating powder* which Hare made as follows: an equivalent of lime was mixed with an equivalent and one-half of "bycyanide of mercury," and the mixture introduced into a porcelain crucible placed in an air-tight alembic of iron "so as completely to exclude atmospheric air." The whole was exposed to a red heat. The residual mass was dissolved in acetic acid, the solution filtered, and mercurous nitrate added to the liquid. The precipitate was washed and "when well dried was found to constitute a powder capable of fulminating by percussion (1839)."

Look at that splendid specimen of *artificial camphor* which Hare got in 1839 by "impregnating oil of turpentine" with dry hydrochloric acid gas! The pinene present in our American oil of turpentine, when acted on with the dry gas, is responsible for this synthesis.

And there is a sample of *alkanet* in that tube. This it was that Hare suggested should be used as an indicator in alkalimetric and acidimetric determinations (p. 109).

The sample of *ethyl perchlorate*, indicated by him as present in another vial, was made, you recall, by his son, Clark Hare (p. 352) in collaboration with Martin Boyè.

Indeed, on all sides there are evidences preserved of the activity of former assistants and pupils. Among these men may be mentioned Franklin Bache, who rendered frequent and able service in experimental work and often assisted in Hare's literary labors, for we are reminded that he saw at least one edition of the *Compendium* through the press when Hare was absent in Europe. There was also D'Wolf, professor of chemistry in Brown University Medical School, whose "knowledge of Chemistry was acquired under the celebrated chemist—Robert Hare." Dr. D'Wolf is reported to have been a brilliant lecturer. And George T. Bowen who, under Silliman's direction, had studied the magnetic effects of the calorimotor, came to Philadelphia to profit by

Hare's instruction. He completed the medical course at the same time. He was also a relative of Mrs. Hare. He subsequently became professor of chemistry in the University of Tennessee, Nashville. Robert E. Rogers was another who, under Hare's inspiration, executed some really notable work upon osmosis. While Martin Boyè, a Swede, subsequently professor of chemistry in the Central High School of Philadelphia, engaged in noteworthy researches with Hare and the latter's son. Just what Wolcott Gibbs did in his sojourn in Hare's laboratory is not known. He must have at least received genuine inspiration from his great colleague from which he and others later profited greatly. And so, many more might be here cited to show that Hare had created a genuine centre of chemical research and thought, to which the serious-minded young scientists of the country turned their eyes. The evidences of all this are apparent in the vast amount of original material in the way of apparatus and preparations which was amassed in the years of Hare's greatest activity.

Having revelled among the marvellous collections in the lecture room and laboratory and having listened to words from the great investigator, we take our departure with expressions of gratitude wholly inadequate for the pleasure which has been ours and wend our way silently homewards, thinking, thinking—of the great privilege which had been granted us.

If to-day—years, long years afterward—we should wish to pay a real visit to the places we have in imagination left, it could not be done. The buildings in which were those wonderful collections have disappeared. Even the collections have vanished. All is gone. It occurred in this manner:

When Hare in 1847 resigned his professorship, the apparatus accumulated by him “was replaced by another apparatus belonging to my successor.” Thereupon Joseph



Henry, Secretary of the Smithsonian Institution, suggested (1848) that Hare should offer his collections to that institution, saying:

“Several of the articles belong to the history of the science of our country and would be interesting mementos of the past which should be preserved in some public institution.”

To which Hare replied: “that it would be agreeable to me to comply with the proposal, it being understood that the cost of the removal of the apparatus and of its being put in good order should be defrayed by the Institution, so that while on the one hand I should receive nothing, on the other, I should not be at any expense; also that suitable apartments and cases should be provided for the keeping and using of the apparatus for the purpose of investigation and illustration.”

The Smithsonian accepted the terms. It was left to Henry to reject all that might be of no use to the Institution, for as Hare said: “I did not deem it proper that I should determine how far articles, which I had preserved under the idea of a contingent utility, might be worthy of the cost of transportation and of the space which they would occupy in the buildings of the Institution.” The apparatus was packed up in the summer of 1847.

In reports of the Smithsonian Institution it appears that rooms were to be set aside for chemical and physical apparatus; “that the room on the east connected with the museum had been fitted up with cases in which to deposit the collection of apparatus presented by Dr. Hare . . . which was interesting on account of its association with the history of the advance of science in this country. The collection contained most of the articles invented by the donor, and which are described in the scientific journals of the first half of the present century (19th). Among the chemical implements were those used by that distinguished chemist, in procuring for the first time, without the aid of galvanism, calcium, the metallic basis of lime.”

Younger generations of chemists in his own university have invariably expressed the deepest regret that Hare should have made the preceding transfer of his scientific treasures. Quite recently, when the writer approached the present Secretary of the Smithsonian for possible data—letters and the like,—concerning Hare, he was grieved to learn that all but a few pamphlets had perished in a fire which, some years ago, took place in the museum. And thus was sustained an irreparable loss, regretted at this moment. No wonder then that the old *Compendium* is so precious to all who care to know something of the history of scientific, experimental endeavor in this country. Let us cherish it then and when in our human conceit we fancy that only the present is worth the while, turn to its pages and there behold that in many things to-day we were anticipated and that our original ideas were long ago formulated by at least one pioneer of science. To confirm this assertion hear what another American chemist—one who has wrought and wrought well,—deeply esteemed and honored at home and abroad,—said in 1915:

“ I became interested in double halides and published an article giving my views regarding the nature of these compounds. Soon after the appearance of my article I received a letter from Dr. Wolcott Gibbs telling me that Robert Hare had expressed similar views in 1821. He sent me his copy of Hare’s Chemistry and I was astonished to read the chapter that had been written fifty or sixty years before my article. The line of thought was practically identical with mine, and it was expressed beautifully. . . . Hare was both investigator and scientific philosopher. IRA REMSEN.”

And in the report of the Smithsonian Institution for 1849 appeared this resolution:

“ The following gentlemen having been recommended by the Regents and officers of the Institution, and being duly



considered by this meeting, were, on motion of Mr. Meredith, unanimously elected honorary members of the Smithsonian Institution, viz.:

Dr. Robert Hare, of Philadelphia,  
Albert Gallatin, of New York,  
Dr. Benjamin Silliman, of Connecticut,  
Washington Irving, of New York."

Hare, from the words of friends, was a congenial companion and while engrossed in the solution of some of the most perplexing problems of physical science, nevertheless took every occasion to be present at all social functions frequented by the leaders in the various walks of professional, civil and scientific life. Among others he was an interested member of the famous "Wistar Party," having for its members the leaders among men in Philadelphia, all of whom were obliged as a first requisite to be members of the American Philosophical Society.

The following invitation carefully indited upon a single sheet of note paper, found in the archives of the Ridgway Library, indicates Hare's membership in this organization of genial, keen, intelligent gentlemen:

D<sup>r</sup> Hare requests the Pleasure  
of D<sup>r</sup> Rushs company on Saturday  
evg next

March 13<sup>th</sup> 1832

Wistar Party

The eminent surgeon, Samuel D. Gross, wrote of Hare:

"In his old age I used to see him at the Wistar Parties. He was a grand specimen of the *genus homo*, with broad shoulders and an immense head. If his brain had been examined, its weight would probably not have been below that of Cuvier, Humboldt or Webster. He read much, thought much, talked much. Upon almost every subject he possessed a fund of information . . . a man of capacious intellect."

In his busiest days he found time to dwell upon themes not scientific. For instance, in 1834, he reverted to ideas he had years before set forth and now expanded them in an "Essay on Credit," in the preface of which appear these very striking and interesting words:

"It is well known that the vocation of the author, and his predominant taste for the cultivation of science, are irreconcilable with political life," but he was nevertheless prevailed upon to discuss his subject "to obviate financial objections to the creation of a Navy," and, therefore, in this second essay he aimed to prove:

"That Credit is an original medium of commercial interchange, constituting in fact a species of money.

That "paper credit" has been erroneously considered as the representative of gold and silver money, although those metals, as the only satisfactory and accessible test of paper money, are to a certain extent necessary to give it currency.

That while it is true that in this respect, and in some others, gold and silver may perform services to which credit is incompetent, it is equally demonstrable that credit may avail under circumstances, in which the precious metals may either be incompetent, or unattainable.

That credit is especially the money of the *honest and industrious*, whether *mechanics, traders, or cultivators of the soil*.

*That Banks give an extension to the credit of men of small capital*, which enables them to deal with persons to whom their credit would be unknown, and thus to acquire the means of employing the labouring class.

That as state banks are useful in performing services to which the credit of individuals is incompetent, so a national bank is advantageous in reaching cases to which state banks are incompetent.

That banks are useful in supplying a more convenient, and less expensive currency than coin.



That credit not only enables the capital of one part of our country, to promote the industry and improvement of other parts, it also enables us by the sale of bank shares, or certificates of state, or national, debt in foreign countries, to enrich our country by all the difference between the profits of the capital thus obtained, and the interest paid to the foreign stockholders.

That the blessings arising from the great mean of public prosperity, which forms the subject of this essay, are dependent on the competency, supremacy, and stability of the laws.

That in this respect the employment of credit as a means of commercial interchange, has a happy influence in associating the pecuniary interest of the great mass of the community, with the cause of good morals, public order, and true liberty."

In 1837 Hare was carried into a discussion—political in character. He evidently had been persuaded by others, for it seems to have been a period of pamphleteering. Thus men like J. Dymond wrote "An Inquiry Into the Accordancy of War with the Principles of Christianity," and William M. Gouge, Albert Gallatin, Alexander Hamilton and others were intensely occupied with the banking system. Hare's contribution now bore the title, "Suggestions Respecting the Reformation of the Banking System."<sup>3</sup> The document was addressed to Mahlon Dickerson, Esq., Secretary of the Navy, who had expressed a wish to be made acquainted with Hare's views on the reformation of the currency. After indulging in preliminary observations, Hare considers the three principal functions of the banking business—the issue of bills, loaning of money, and receiving and holding deposits subject to order. These functions he held were not inseparable, and urged that they ought not to be united. It was a national prerogative to issue bills or notes, while the other functions

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<sup>3</sup> Philadelphia, printed by John C. Clark, No. 60 Dock St.

were inherently a private right. The hostility to banks was in truth directed against their issue of notes. He thought that if banking was a disease, then excision was a too heroic treatment, something milder was needed. It was inexpedient to restrict offices of discount and deposit if the right to issue bills was not assumed. The association of the function of loaning money on personal security with that of the issuance of notes was pregnant with evil. It is a national duty to create and support a good and stable currency. Specie is a proper *measure* of value, but not an adequate *basis* for a currency. Specie is an article of merchandise, and liable to be abstracted in order to restore the balance of trade in other articles. National credit is a more secure basis. The nation, therefore, should guarantee its currency. In 1791 only three banks existed to be curbed; now (1837) more than seven hundred were to be curtailed in their circulation. State banks should resolve themselves, in their separate capacities, into offices of discount and deposit, and associate jointly for the guarantee of their bills. Each bank should contribute its circulation and a proportionable part of its capacity, to a trust fund, receiving a corresponding amount of joint bills or notes for the value. The loaning of national funds to speculators is a source of peril to the treasury, and of demoralization and misery to the public.

These and kindred ideas were set forth by Hare in rather vigorous style, marked with large views and elevated patriotism.

In the third decade of the 19th Century chemistry flourished in all European countries to a marvelous degree. The French and German representatives were contributing largely to theory as well as to experiment. Dumas and Liebig were heard at frequent intervals on constitution of substances, but the great Nestor of the north, Berzelius, dominated the inorganic field at least, with his electrochemical



or dualistic theory. These theories—these views—relative to constitution were quite familiar to Hare. He never lost sight of current literature, and pondered most thoughtfully upon it. His correspondence with Berzelius gave him accurate information, illuminated for him the many points of discussion, which arose from time to time. Surely, then the following communication ought to be welcome to every student of the development of chemical theory. It is a question as to whether any other American chemist of that period, among those who were doing investigation, however primitive, gave as much consideration to theoretical subjects as did Hare. His letter appears in full. At this remote day it brings food for thought:

“ My dear Silliman:

“ Philad<sup>a</sup> March 28<sup>th</sup>

I wrote to you about a month since enclosing an abstract from the London & Edinburg Philosophical magazine and journal to be inserted in your American journal of Science. Since then I have not heard from you. I send to you by this mail a copy of my article on the Berzelian nomenclature. It was fully proved that I should have been wrong to send it away from here to be printed for between my own errors and those of the printers, I had at least four proofs before it was made satisfactory.

I found some errors in the translation of Berzelius's letter 12 line page 6<sup>th</sup> which read “ as well as those of sulphuric acid ” instead of as well as sulphuric acid or more properly as in copy now sent.

7<sup>th</sup> line same page is altered to make it begin a sentence.

Line 4 same page it reads shall then have for.

There is a change made in the 74<sup>th</sup> line same page. There are others not very important.

I have recd from M<sup>r</sup> Clarke a lot of electro magnetic apparatus which has cost me about thirty dollars. It consists of Mullin's battery and Rubies apparatus by which a

magnet is made to revolve with very little power as long as the chemical reaction is sustain'd also an electripeter by which the current is reversed with great facility. A double set of Ampers and Marshs rotatory Cylinders. Also another set of rotatory apparatus. If you wish it I will send you the whole at cost as I shall be enabled to replace them before my course begins.

I have heard from my family as late as the sixth of Feb' all well. I expect to see them before the end of the summer.

We have just finished our course and examinations having passed about 145 in all—

Please to send me by mail a copy of whatever you print of mine or affecting me as soon as possible after it is struck off—

Faithfully

Your f<sup>d</sup>

ROB<sup>t</sup> HARE ”

“ My dear Silliman,

“ Philadelphia, June, 1834.

I have already apprized you, that last year I had the honour to receive from the celebrated Berzelius, six volumes of his admirable treatise of Chemistry; to which, during the last summer, I gave much time, in order to avail myself of the vast fund of useful practical knowledge which it contains. I am of opinion that to adepts in the science, this treatise is the most interesting and instructive compilation of chemical knowledge which has ever issued from the press. It comprises much matter for which Chemistry is indebted entirely to the genius, skill, and industry of the author, while scarcely any subject in it is so treated, as not to create a renovated interest in the reader, however previously familiar with the science.

Sweden may with reason be proud of her Scheele, her Bergman, and her Berzelius. The last, but not the least, of these great chemists, aided by an Herculean intellect, and



commencing at the point at which his predecessors terminated their glorious career, may be considered as possessing attainments which have never been excelled. Yet the sun is not without spots, nor is Berzelius without errors; unless indeed, those which I have ascribed to him, are phantoms of my own intellectual vision.

I concur with those chemists who consider the relation ascertained by Berzelius, between the quantities of oxygen in oxybases, and in oxacids, as a necessary consequence of the laws of combination, on which the Daltonian theory has been founded. I conceive also that the interesting facts which demonstrate the existence of the relation alluded to, would be more easily understood and remembered, if referred to the theory of atoms, than when made the basis of his doctrine of capacities for saturation, and of the numbers by which those capacities are expressed.

Moreover, I do not approve of his nomenclature. This is a subject highly interesting to me at this time. The last edition of my text book is exhausted, and in publishing a new edition I shall be obliged either to adopt the nomenclature of Berzelius, or to adhere to that now generally used, with such improvements as may seem to me consistent with its principles.

I will proceed to state my objections to the Berzelian nomenclature, and to suggest the language which I would prefer. I should be glad if the promulgation of my opinions should call forth remarks, which may enable me to correct, in due season, any errors into which I may have fallen. I regret the necessity of making a final election, before submitting my objections to Berzelius himself, whose disapprobation it would grieve me much to incur.

My apology will be found in the adage—"Amicus Plato sed magis amica veritas." Besides, if my opinions are incorrect, they will only react upon their author. The productions of Berzelius stand deservedly too high in public favour to be reached by ill founded criticism.

The most striking feature in the nomenclature of Berzelius, is the formation of two classes of bodies; one class called "halogene," or salt producing, because they are conceived to produce salts directly; the other called "amphigene," or both producing, being productive both of acids and bases, and of course indirectly of salts. To render this division eligible, it appears to me that the terms acid, base, and salt, should, in the first place, be strictly defined. Unfortunately, there are no terms in use, more broad, vague, and unsettled in their meaning. Agreeably to the common acceptation, chloride of sodium is pre-eminently entitled to be called a salt; since in common parlance, when no distinguishing term is annexed, salt is the name of that chloride. This is quite reasonable, as it is well known that it was from this compound, that the genus received its name. Other substances, having in their obvious qualities some analogy with chloride of sodium, were, at an early period, readily admitted to be species of the same genus: as, for instance, Glauber's salt, Epsom salt, sal ammoniac. Yet founding their pretensions upon similitude in obvious qualities, few of the substances called salts, in the broader sense of the name, could have been admitted into the class. Insoluble chlorides have evidently, on the score of properties, as little claim to be considered as salts, as insoluble oxides. Luna Cornea, plumbum corneum, butter of antimony, and the fuming liquor of Libavius, are the appellations given respectively to chlorides of silver, lead, antimony, and tin, which are quite as deficient of the saline character as the corresponding compounds of the same metal with oxygen. Fluoride of calcium (fluor spar) is as unlike a salt as lime, the oxide of the same metal. No saline quality can be perceived in the soluble "haloid salts," so called by Berzelius, while free from water; and when a compound of this kind is moistened, even by contact with the tongue, it may be considered



as a salt formed of an hydracid and an oxybase, produced by a union of the hydrogen of the water with the halogene element, and of the oxygen with the radical. It is admitted by Berzelius, vol. 3, page 330, that it cannot be demonstrated that the elements of the water, and those of an haloid salt, dissolved in that liquid, do not exist in the state of an hydracid and an oxybase, forming a salt by their obvious union.

On the other hand, if instead of qualities, we resort to composition as the criterion of a salt; if, as in some of the most respectable chemical treatises, we assume that the word salt is to be employed only to designate compounds consisting of a base united with an acid, we exclude from the class chloride of sodium, and all other "haloid salts," and thus overset the basis of the distinction between "halogene" and "amphigene" elements.

Moreover, while thus excluding from the class of salts, substances which the mass of mankind will still consider as belonging to it, we assemble under one name combinations opposite in their properties, and destitute of the qualities usually deemed indispensable to the class. Thus under the definition that every compound of an acid and a base, is a salt, we must attach this name to marble, gypsum, felspar, glass, and porcelain, in common with Epsom salt, Glauber's salt, vitriolated tartar, pearlash, &c. But admitting that these objections are not sufficient to demonstrate the absurdity of defining a salt, as a compound of an acid and a base, of what use could such a definition be, when, as I have premised, it is quite uncertain what is an acid, or what is a base. To the word acid, different meanings have been attached at different periods. The original characteristic sourness, is no longer deemed essential! Nor is the effect upon vegetable colours treated as an indispensable characteristic. And as respects obvious properties, can there be a greater discordancy, than that which exists between sulphuric acid, and rock crystal; between

vinegar, and tannin; or between the volatile, odoriferous, liquid, poison, which we call prussic acid, and the inodorous, inert, concrete, material for candles called margaric acid?

While an acid is defined to be a compound capable of forming a salt with a base, a base is defined to be a compound, that will form a salt with an acid. Yet a salt is to be recognized as such, by being a compound of the acid and base, of which, as I have stated, it is made an essential mean of recognition.

An attempt to reconcile the definitions of acidity given by Berzelius, with the sense in which he uses the word acid, will in my apprehension, increase the perplexity.

It is alleged in his Treatise, p. 1, Vol. II, "That the name of acid is given to silica, and other feeble acids, because they are susceptible of combining with the oxides of the electropositive metals, that is to say, with salifiable bases, and thus to produce salts, which is precisely the principal character of acids." Again, Vol. I, page 308, speaking of the halogene elements, he declares that "Their combinations with hydrogen, are not only acids, but belong to a series the most puissant that we can employ in Chemistry; and in this respect they rank as equals with the strongest of the acids, into which oxygen enters as a constituent principle." And again, Vol. II, page 162, when treating of hydracids formed with the halogene class, he alleges, "The former are very powerful acids, truly acids, and perfectly like the oxacids; but they do not combine with salifiable bases; on the contrary, they decompose them, and produce haloid salts."

In this paragraph, the acids in question are represented as pre-eminently endowed with the attributes of acidity, while at the same time they are alleged to be destitute of his "principal character of acids," the property of combining with salifiable bases.

In page 41 (same volume), treating of the acid consist-



ing of two volumes of oxygen and one of nitrogen, considered by chemists generally as a distinct acid, Berzelius uses the following language: "If I have not coincided in their view, it is because, judging by what we know at present, the acid in question cannot combine with any base, either directly or indirectly, that consequently it does not give salts, and that salifiable bases decompose it always into nitrous acid, and nitric oxide gas. It is not then a distinct acid, and as such ought not to be admitted in the nomenclature." Viewing these passages with all that deference which I feel for the productions of the author, I am unable to understand upon what principle the exclusion of nitrous acid from the class of acids, can be rendered consistent with the retention, in that class, of the compounds formed by hydrogen with "halogene" elements.

Having thus endeavored to show that the words acid, salt, and base, have not been so defined as to justify their employment as a basis of the Berzelian nomenclature I will with great deference proceed to state my objections to the superstructure, erected upon this questionable foundation. Consistently with the French nomenclature, the combinations formed by electro-negative principles, with other elements, have been distinguished as acids, or characterized by a termination in "ide" or in "ure" which last monosyllable, when there has been no intention of altering the meaning, has, by the British chemists, been translated into uret. The termination in ide, which is common in both languages, is, by Thenard, and other eminent French authors, restricted to the binary compounds of oxygen, which are not acid. Analogous compounds formed with the "halogene" elements, chlorine, bromine, fluorine, iodine, cyanogen, &c., have by the same writer been designated by the termination in ure. Thus we have in his work, chlorures, bromures, fluorures, iodures, cyanures. Some of the most eminent chemists in Great Britain, have distinguished the elements called halo-

gene, by Berzelius, together with oxygen, as supporters of combustion; and have designated the binary compounds made with them, when not acid, by the same termination as the analogous compounds of oxygen. Accordingly in their writings, instead of the names above mentioned, we have chlorides, bromides, fluorides, iodides. In Henry's Chemistry, cyanure is represented by cyanide, in Thomson's, by cyanodide, and in Brande's and Turner's by cyanuret.

The term uret, equivalent as above mentioned to the French ure, is restricted by the English chemists to the compounds formed by non-metallic combustibles, either with each other, or with metals. Hence we have in English, sulphurets, phosphurets, carburets, borurets, for sulphures, phosphures, carbures, borures, in French.

Berzelius classes as electronegative, all those substances which go to the positive pole when isolated, or when in union with oxygen, while all substances are by him treated as electropositive which go to the negative pole, either when isolated, or when in union with oxygen.<sup>4</sup>

According to his nomenclature, when both the ingredients in a binary compound belong to the class of bodies by him designated as electronegative, the termination in ide, is to be applied to the more electronegative ingredient; but where

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<sup>4</sup> The term isolated, is employed to convey an idea of the state in which the elements of water are, when after having been separated by the voltaic wires, they are severally on their way to their appropriate poles, that is, the oxygen proceeding to the positive pole, and the hydrogen to the negative pole. Each element is in that case isolated, and obedient to the attractive influence of one of the poles. When a salt containing an oxacid and an oxybase, is decomposed, the acid will go to the positive and the base to the negative pole. The radical of the acid, in consequence of its not counteracting the propensity of the oxygen for the positive pole, is deemed electronegative; while the radical of the base overcoming that propensity is deemed electropositive.



one of the ingredients belongs to his list of electropositive bodies, the termination in ure (uret, in English), is to be applied to the electronegative ingredient. As, agreeably to the prevailing nomenclature, which in this respect, the great Swedish chemist has not deemed it expedient to change, the electropositive compounds of oxygen with radicals, forming electropositive bases, have each a termination in ide, it seems that consistence requires us, conformably with the English practice, to designate in like manner analogous electropositive compounds of the electronegative elements called by him "halogene." But especially it would be inconsistent not to put the same mark upon the compounds of substances which from their analogy with oxygen are placed in the same "amphigene" class. If there were insuperable reasons for retaining the term oxide, as a generic name for the electropositive compounds of oxygen, it seems to me inexpedient not to employ the words sulphide, selenide, and telluride, to designate the electropositive compounds of sulphur, selenium, and tellurium. And since the three last mentioned elements when united with hydrogen, form electronegative compounds which act as acids, why not treat them as such, under appellations corresponding with those heretofore used for that purpose? I conceive the following definitions to be justified by the practice of modern chemists in general, as established in the case of oxacids, and oxybases. When two compounds capable of combining with each other to form a tertium quid, have an ingredient common to both, and one of the compounds prefers the positive, the other the negative pole of the voltaic series, we must deem the former an acid, and the latter a base. And again, all compounds having a sour taste, or which redden litmus, should be deemed acids in obedience to usage.

I should think it preferable, if in adopting these definitions, the termination in ide was considered as applicable to

all compounds of electronegative principles with other substances, whether producing electronegative or electropositive combinations, and that the terms acid, and base, should be considered as severally indicating the subordinate electronegative and electropositive compounds. In that case oxybase, chloribase, fluobase, bromibase, iodobase, cyanobase, sulphobase, telluribase, selenibase, would stand in opposition to oxacid, chloracid, fluacid, bromacid, iodacid, cyanacid, sulphacid, selenacid, telluracid; yet for convenience, the generic termination *ide* might be used without any misunderstanding; and so far, the prevailing practice might remain unchanged. Resort to either appellation would not, agreeably to custom, be necessary in speaking of salts or other compounds analogous to them; since it is deemed sufficient to mention the radical as if it existed in the compound in its metallic state. Ordinarily we say, sulphate of lead, not sulphate of the oxide of lead. This last mentioned expression is resorted to, only where great precision is desirable. In such cases, it might be better to say sulphate of the oxybase of lead. So long however as the electronegative combinations of oxygen are designated as oxacids, and the electropositive as oxides, it seems to be incorrect, not to use analogical terms in the case of analogous compounds, formed by the other pre-eminently electronegative principles and assuming the definition above stated, to be justified by modern practice, it follows that in order to entitle the electronegative and electropositive ingredients of the double salts of Berzelius, to be classed, the latter as bases, and the former as acids, it is not necessary to appeal to the highly interesting and important experiments of Bonsdorf, confirmed in some instances by the testimony of Berzelius himself, proving that the attributes of acidity (as heretofore defined) exist in the one case, and those of alkalinity in the other. My definition is founded upon the conviction that these characteristics have not lat-



terly been deemed necessary to acids, and that in bases they never were required; having, as respects them, only served as a means of subdivision, between alkaline oxides and other bases.

Chemistry owes to Berzelius much valuable information respecting the compounds formed by the substances which he calls "halogene"; especially respecting the combinations formed by fluorine, with boron, and silicon, and the "double salts," as he considers them, formed by the union of two "halogene salts," &c. While in the highest degree interested in the facts which he has ascertained, it will be inferred from the premises, that I do not perceive that any adequate line of distinction can be drawn in this respect between the simple salts formed by oxacids and oxybases; and the double salts formed by his "halogene" elements.—Agreeably to the definition which I have ventured to propose, in a combination of this kind, the electronegative salt would play the part of an acid, while the electropositive salt would perform that of a base.

In common with other eminent chemists, he has distinguished acids in which oxygen is the electronegative principle, as oxacids, and those in which hydrogen is a prominent ingredient as hydracids. If we look for the word radical, in the table of contents of his invaluable Treatise, we are referred to p. 218, vol. I, where we find the following definition, "the combustible body contained in an acid, or in a salifiable base, is called the radical of the acid, or of the base." In the second vol., page 163, he defines hydracids to be "those acids, which contain an electronegative body, combined with hydrogen"; and in the next page it is stated, that "hydracids are divided into those which have a simple radical, and those which have a compound radical. The second only comprises those formed with cyanogen and sulphocyanogen." Again, in the next paragraph, "no radical is known that gives more than one acid with hydrogen, although sulphur and

iodine, are capable of combining with it in many proportions. If at any future day more numerous degrees of acidification with hydrogen, should be discovered, their denomination might be founded on the same principles as those of oxacids."

Consistently with these quotations, all the electronegative elements forming acids with hydrogen, are radicals, and of course by his own definition, combustibles; while hydrogen is made to rank with oxygen as an acidifying principle, and consequently is neither a radical nor a combustible. Yet page 189, vol. II, in explaining the reaction of fluoboric acid with water, in which case fluorine unites both with hydrogen and boron, it is mentioned as one instance among others in which fluorine combines with two combustibles.

I am of opinion that the employment of the word hydracid, as co-ordinate with oxacid, must tend to convey that erroneous idea, with which, in opposition to his own definition, the author seems to have been imbued, that hydrogen in the one class, plays the same part as oxygen in the other. But in reality, the former is eminently a combustible, and of course the radical, by his own definition.

Dr. Thomson, in his system, does not recognize any class of acids, under the appellation of hydracids; but with greater propriety, as I conceive, places them under names indicating their electronegative principles. Thus he arranges them as oxygen acids, chlorine acids, bromine acids, iodine acids, fluorine acids, cyanogen acids, sulphur acids, selenium acids, and tellurium acids.\* Those appellations might, I think, be advantageously abbreviated into oxacids, chloracids, fluacids, bromacids, iodacids, cyanacids, sulphacids, selenacids, telluracids.

As respects the acids individually, I conceive that it would be preferable, if the syllable indicating the more electronegative element had precedency in all, as it has in some cases.

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\* I had formed my opinions on this subject, before I was aware that Dr. Thomson had resorted to this classification.



The word hydrofluoric does not harmonize with fluoboric, fluosilicic, fluochromic, fluomolybdic, &c. Fluorine being in each compound the electronegative principle, the syllables indicating its presence, should in each name occupy the same station. These remarks will apply, in the case of acids formed with hydrogen, to all principles which are more electronegative. Hence we should use the terms chlorohydric, fluohydric, bromohydric, iodohydric, cyanhydric, instead of hydrochloric, hydrofluoric, hydrobromic, hydriodic, hydrocyanic.

These opinions, conceived last summer, were published by me in the *Journal of Pharmacy* for October last. Since then, I find that in the late edition of his *Traité*, Thenard has actually employed the appellations above recommended.

As by the British chemists the objectionable words have not been definitely adopted; the appellations muriatic and prussic, being still much employed, it may not be inconvenient to them to introduce those which are recommended by consistency. In accordance with the premises, the acids formed with hydrogen by sulphur, selenium, and tellurium, would be called severally sulphydric, selenhydric, and telluhydric acid. Compounds formed by the union of the acids thus designated, with the bases severally generated by the same electronegative principles, would be called sulphhydrates, selenhydrates, and telluhydrates, which are the names given to these compounds in the Berzelian nomenclature. Influenced by the analogy, a student would expect the electronegative ingredient of a sulphhydrate to be sulphydric acid, not a sulphide. The terminating syllable of this word, by its associations, can only convey the conception of an electro-positive compound.

By adhering to the plan of designating each acid by its most electronegative ingredient, the compounds of hydrogen and silicon, or of hydrogen and boron with fluorine, would

appear in a much more consistent dress. In the compound named hydrofluoboric acid, and that named hydrofluosilicic acid by Berzelius, fluorine is represented as acting as a radical with hydrogen, while with boron and silicon it acts as the electronegative principle. It has been shown that hydrogen, no less than boron and silicon, must be considered as a combustible, and of course a radical. This being admitted, if the compounds in question are really entitled to be considered as distinct acids, their names should respectively be fluohydroboric, or fluohydrosilicic acid. But as I have elsewhere observed an incapacity to combine with bases, or to react with them without decomposition, is made by Berzelius an adequate reason for expunging the compound formed by one atom of nitrogen with four atoms of oxygen from the list of the acids of nitrogen; I do not, therefore, understand how the compounds referred to, while equally incapable of combination, can be considered by him as acids. At first it struck me that the liquids consisting of fluohydric acid, either with fluoboric acid, or with fluosilicic acid, might be considered as merely united by their common attraction to water, since they separate when this liquid is abstracted by evaporation. Upon reflection, however, I retract that opinion, since it appears to me that if the compounds in question are to be considered as acids, they may be viewed satisfactorily as fluacids with a double radical; but I deem it more consistent to suppose that a fluobase of hydrogen in the one case united with fluoboric acid, in the other, with fluosilicic acid; so that fluohydroboric acid might be called fluoborate of the fluobase of hydrogen, or more briefly fluoborate of hydrogen; and in like manner fluohydrosilicic acid would be called fluosilicate of the fluobase of hydrogen, or briefly fluosilicate of hydrogen.

There are instances in which compounds, usually called bases, act as acids. Of course it is consistent that compounds, usually called acids, should in some instances act as bases.



In this respect a striking analogy may be observed between the union of the oxide of hydrogen (water) with the oxacids and oxybases; and that of fluoride of hydrogen with fluacids and fluobases. According to Berzelius, water, in the first case, acts as a base, in the second as an acid. So I conceive the fluoride of hydrogen acts as a base in the cases above noticed, while it acts as an acid in the compound of hydrogen, fluorine, and potassium, called by Berzelius "fluorure potassique acide." This compound I would call a fluohydrate of the fluobase of potassium, or more briefly fluohydrate of potassium, as we say sulphate of copper, instead of the sulphate of the oxide (or oxybase) of copper. It appears from the inquiries of the author of the nomenclature under consideration, that each of the three acids above mentioned as formed by fluorine, with the three different radicals, hydrogen, boron, and silicon, is capable with electropositive metallic fluorides, of forming the compounds treated of by him as double salts. These compounds, to which I have already alluded, might be called fluohydrates, fluoborates, or fluosilicates of the metallic ingredient. As for instance, the compound into which potassium enters, named by him "fluorure borico potassique," I would designate as a fluoborate of the fluoride (or fluobase) of potassium, or for the sake of brevity, fluoborate of potassium. "Flourure silico-potassique" would by the same rule, be called fluosilicate of potassium.

The illustration thus given in the instance of potassium, renders it unnecessary to furnish other examples, as it would only require that the name of any other metal should be substituted for that of potassium, in order to modify these appellations, so as to suit every case.

Pursuant to my fundamental definition, ferroprussiate of potash, cyanure ferroso potassique in the Berzelian nomenclature, should be considered as a compound of cyano-ferric acid, and a cyanide or cyanobase of potassium and

would of consequence be a cyanoferrate of potassium. Or if the iron be in two different degrees united with cyanogen, as the names cyanure ferroso potassique, and cyanure ferrico potassique indicate, we should have both a cyanoferrite and a cyanoferrate of potassium; and of course cyanoferrous and cyanoferric acid for their respective electronegative ingredients. "Cyanure ferrique acide" would be exchanged for cyanoferrate of hydrogen, being a case analogous to that of the "fluorure potassique acide" above considered and provided for.

If I am justified in my impression above stated, water, and the compound formed by fluorine with hydrogen ("hydrofluoric acid" or fluohydric acid as I prefer to call it) should be severally designated as acids when they act as acids; as bases, when they act as bases. In other cases the one might be designated as an oxide, the other as a fluoride of hydrogen. In the case of a compound so well known as water, I would adhere to the common name, resorting to the scientific names only as definitions. Thus water would be defined as an oxide of hydrogen, which in some combinations, acts as an oxybase of hydrogen, in others as hydric acid, or the oxacid of hydrogen.<sup>5</sup>

After designating as metalloids all non-metallic bodies, Berzelius alleges (page 203, vol. 1st) that they are divided into oxygen, and bodies which are combustible, or susceptible of combining with oxygen; in which process the greater part display the ordinary . . . phenomena of combustion, or, in other words, of fire. Agreeably to this classification, susceptibility of union with oxygen and combustibility are con-

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<sup>5</sup> The use which I have made of the terminations in *ide*, in fluoride of hydrogen, or oxide of hydrogen, to signify a compound of hydrogen with fluorine, or oxygen generally, without conveying the idea of its being either a base or an acid, illustrates the advantage which would result from the use of that termination in that broad sense.



founded; to which I object, because oxidizement frequently ensues without combustion, and combustion occurs often without oxidizement.

Speaking of chlorine (Treatise, p. 276, vol. 1) it is alleged that it supports the combustion of a great number of bodies, of which a majority ignite in it at ordinary temperatures. If oxidizement be identical with combustion, how can this word be employed with propriety in the case thus quoted, where oxygen is not present? If combustion in the case of chlorine is applied only to those instances in which reaction with other bodies is attended by the phenomena of fire, why is not the term equally restricted in its application in the case of oxygen?

Oxygen differs so far from the substances usually called combustibles, that they will produce fire with oxygen, and with but few, if any other substances; while oxygen will produce fire with many substances. But this characteristic of producing fire with many substances applies to chlorine, and as chlorine does not produce fire with oxygen, it is devoid of the only characteristic which should entitle it to be treated as a combustible, if combustibility and susceptibility of union with oxygen be identical.

Hence, if it be deemed proper in the case of oxygen to place the bodies with which it enters into combustion in one class, designated as combustibles, while oxygen is distinguished as the common "comburant" of them all, there is equal reason for placing chlorine in a like predicament. The impropriety of designating the substances comprised in his halogene and amphigene classes, with the exception of oxygen as combustibles, upon the basis of their susceptibility of oxidizement, must be evident from the fact that fluorine is not oxidizable, while it is so perfectly analogous to the others, especially chlorine, in its properties, that it would be disadvantageous to call it apart.

Berzelius objects to the use of the word "comburant"

(equivalent to the English word supporter), upon the ground that the same substance may be alternately a supporter and a combustible. I should, however, go farther, and likewise object to the use of both words, as tending to convey the erroneous impression, that in combustion, one of the ponderable agents concerned, performs a part more active than the other; whereas, in all such cases, the reaction must evidently be reciprocal and equal. I have repeatedly shown to my pupils, that a jet of oxygen burns in an atmosphere of hydrogen, as well as a jet of hydrogen similarly situated in oxygen.

I would recommend that all the bodies comprised in the halogene and amphigene classes of Berzelius, should be placed under one head, to be called the basacigen class; thus indicating their common and distinguishing quality agreeably to the premises, of producing both acids and bases. The electro-negative compounds of these substances to be called acids, their electropositive compounds, bases, as already suggested.<sup>6</sup>

Faithfully,

Yr friend,

ROBT. HARE.

Hare was in no manner disposed to discontinue his critical views of the ideas set forth by Berzelius. They, therefore, constitute a part of the history of our Science and cannot fail to attract the student of the history of chemical theory. It is not surprising that Berzelius may have been at times mildly displeased with the persistency of Hare in setting forth his views for, on a certain occasion when writing to Silliman, on other subjects, he said:

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<sup>6</sup> Since the preceding letter was ready for the press the following remark of Berzelius attracted my attention, as sanctioning indirectly the definition which I have proposed, page 5.

Treatise, Vol. 3, page 323, he alleges, "It follows from this that the property of playing the part of an acid, is attached neither to the substance, nor to the manner in which the combination takes place. It only indicates a state contrary to the property of being a base.



“Present my respects to our friend Mr. Hare. I owe him a long controversial letter on scientific matters. . . . It is a little hazardous to enter into private discussion with this savant, because he immediately prints all that is written to him, followed by a refutation. I have sometimes been surprised to read in your Journal a reply to my ideas which I had never seen except there. One cannot be angry, however, for Mr. Hare is a good man, and seeks the truth before everything; but that makes me desire not to turn a private controversy into a public one. But much depends on the habits of different countries ” . . .

And as a reply to Hare's communication respecting nomenclature (p. 222) he received from Berzelius this letter:

“ Sir,

“ Stockholm, Sept. 23, 1834.

I am very much obliged to you for the remarks, which, under the date of June 21st, you had the friendship to communicate to me respecting the nomenclature which I have employed in my Treatise of Chemistry.

I perceive that having contemplated chemical phenomena under different points of view, we differ as to the nomenclature which is the most appropriate for their description. I consider the combinations of metals with chlorine, bromine, &c., as salts; whilst you, in accordance with Mr. De Bonsdorff, consider them as bases and acids, capable of forming salts by their union.

If it were expedient that chemical classification should be dependent on the number of simple bodies which enter into each combination, this idea of Mr. De Bonsdorff would without doubt be preferable; but if attention be due to the chemical properties which characterize combinations, we cannot adhere to an arrangement founded on the number of the elements. Yet so essential is it in chemistry to have reference to properties, that a system of chemistry in which common

and analogous properties should not affect the arrangement, would present a mass of facts so chaotic, that no memory would be competent to retain them. In a system thus strictly conformable to the ideas of Mr. De Bonsdorff, cyanogen, though in its properties resembling chlorine or bromine, which are simple bodies, ought to be considered, also, as a base or as an acid, having azote for its radical—I am persuaded you would not approve of extending the system of De Bonsdorff so far; but if it be correct, it would be inconsistent not to make this extension.

But let us return to the combinations of the metals with chlorine, fluorine, &c., and make, in imagination, the following experiment. Let us take two portions of caustic potash, a base in which the *basic* characters are more striking than in any other. To one, let us add a sufficiency of sulphuric acid to extinguish entirely its *basic* property; we shall then have a neutral body of a saline taste. You will admit it to be a salt. Now let us add to the other portion, hydrofluoric acid. At a certain point the *basic* properties of the potash will disappear, and we shall have a resulting compound quite as neutral as the sulphate of potash, endowed with a saline taste entirely analogous to that of the sulphate. The *basic* properties of the potash are destroyed by the hydrofluoric acid, as well as by the sulphuric acid. But you will allege the resulting combination is not a salt, but a base which has exchanged one basifier (oxygen) for another basifier (fluorine). In proof you may add as much more hydrofluoric acid, which combining with the new base will form with it a crystallized salt. But this salt is not neutral, it has almost the same acidity of taste as the hydrofluoric acid employed. The new base does not destroy then the acid reaction.

Let us make a further addition of sulphuric acid to the sulphate of potash. A salt equally acid will result, in which the sulphate of potash acts the same *basic* part towards the



sulphuric acid, as the fluoride of potassium towards the hydrofluoric acid. Should it be desired to extend the comparison further, it will be found that for each less electro-positive fluoride, susceptible of combination with the potassic fluoride, there will be, with but very few exceptions, a corresponding sulphate, susceptible of combination with the sulphate of potash. The analogy is then complete, it exists not only in the perfect neutrality of the two potassic salts, in their saline taste, but also in their manner of forming combinations with other bodies, notwithstanding one of them, the sulphate, contains one element more than the other. If, instead of potash, potassium were employed to saturate our two acids, the analogy of the operation in both cases would be still more complete. The same quantity of metal would displace equal volumes of hydrogen. When the visible results of our experiments are so perfectly analogous, it is to be presumed that the invisible process which we do not see, may also be perfectly analogous, and that if facts exactly like are explained differently, there must be a defect in the explanation. If, for instance, the true electro-chemical composition of the sulphate of potash should not be  $\text{KO} + \text{SO}_3$ , as is generally supposed, but  $\text{K} + \text{SO}_4$ , and it appears very natural that atoms, so eminently electro-negative as sulphur and oxygen, should be associated, we have, in the salt in question, potassium combined with a compound body, which, like cyanogen in  $\text{K} + \text{C}_2\text{N}$ , imitates simple halogen bodies, and gives a salt with potassium and other metals. The hydrated oxacids, agreeably to this view, would be then hydracids of a compound halogen body, from which metals may displace hydrogen, as in the hydracids of simple halogen bodies. Thus we know that  $\text{SO}_3$ , that is to say, anhydrous sulphuric acid, is a body whose properties, as respects acidity, differ from those which we should expect in the active principle of hydrous sulphuric acid.

The difference between the oxysalts, and the halosalts is very easily illustrated by formulæ. In K/FF—fluoride of potassium, there is but one single line of substitution, that is to say, that of K/FF, whilst in KOOOOS (sulphate of potash) there are two K/OOOOS and KO/OOOS of which we use the first in replacing one metal by another, for instance, copper by iron; and the second in replacing one oxide by another.

I do not know what value you may attach to this development of the constitution of the oxysalts (which applies equally to the sulphosalts and others): but as to myself, I have a thorough conviction, that there is therein, something more than a vague speculation; since it unfolds to us an internal analogy in phenomena, which, agreeably to the perception of our senses, are externally analogous. If these phenomena are to be considered agreeably to the ideas of Mr. De Bonsdorff, how does it happen that sulphur, phosphorus, arsenic, and other radicals of the strongest oxacids, when united with chlorine, bromine, iodine, &c., do not combine easily with those of magnesium, iron, and manganese. Should then the chloride of magnesium, or that of manganese, be a stronger acid than the chloride of sulphur, or chloride of phosphorus? How is it consistent with these ideas that we can obtain crystallized salts as well with, as without water, of combination, composed of chloride of calcium and of oxalate, or of acetate of lime? Should the oxysalt be here the acid, or the base? I have now displayed to you, the considerations which have guided me, and which I think are not destitute of foundation.

I cheerfully admit that it would be preferable to employ the word chlorohydric, instead of hydrochloric. My motive for retaining this last, is, that I have ventured to propose a new nomenclature in a language foreign to me, in which it was inexpedient to make changes which could be avoided



without inconvenience. I also agree with you, that we ought not to use combustible and oxidable as having the same meaning. I have deserved your strictures for this inconsistency in my language; but I must suggest as an apology, that the two words were formerly used as synonymous, and that the work, in which you have recently noticed this oversight, was first published in 1806, having been from time to time remoulded for new editions, without having been possible to eradicate all that has not kept pace with the progress of science.

Accept the assurance of my perfect esteem, and of the sentiments of sincere friendship with which I have the honor to be, Yours, &c.

BERZELIUS."

Naturally Hare replied; and in these words, addressed apparently, to the chemical public:

"So far as my strictures were founded on the alleged difficulty of defining the terms acid, salt and base, in any mode consistent with his classification, they are not met by any facts or reasoning in the much esteemed letter of my illustrious correspondent. The impracticability of defining a salt, as he does not deny; and with great candor he admits that, in his definition of acidity, he has not been consistent. He concedes that it would be preferable to give the syllable, indicating the electro-negative ingredient, the precedence, as nothing but unwillingness to innovate, prevented him from pursuing that course.

He acknowledges that as combustion, in many instances, takes place without the presence of oxygen, the application of the word combustible, should not be confined to bodies which are susceptible of oxydizement.

My definition of acidity was as follows:—

*"When, of two substances capable of combining with EACH OTHER SO As To form a tertium quid, and having*

*an ingredient common to them both, one prefers the positive the other the negative pole of the Voltaic series, we must deem the former an acid, and the latter a base, Also all substances having a sour taste, or which redden litmus, must be deemed acids, agreeably to usage."* This definition I would now amend by leaving out the last sentence, and substituting therefor, the following: *Also when any substance is capable of forming a tertium quid with any acid or base agreeably to the preceding definition, it must be considered as an acid in the one case, a base in the other.* The definition, thus amended, takes in the organic acids and bases. In the form in which it was at first proposed, it has not been alleged defective by Berzelius; but he has striven to show an incongruity in the attributes of his double salts, when contrasted with those resulting from the union of some of the acids and bases of his amphigen class; which incongruity is, in his opinion, a sufficient reason for not considering them as simple *salts*, and their ingredients as acids and bases, agreeably to the opinions of De Bonsdorff and myself.

Berzelius errs in confounding my opinions with those of De Bonsdorff. However, I may have admired the sagacity with which that chemist investigated the pretensions of some haloid salts to certain attributes of acidity or alkalinity; in my letter on the Berzelian nomenclature, I signified my unwillingness to rest my opinions upon a basis so narrow, as that which he had endeavored to establish. I stated that I did not deem it necessary to appeal to his excellent observations, proving certain attributes of acidity to exist in one case, those of alkalinity in the other. I alleged my definition to be forwarded on the conviction that the property of affecting vegetable colors, on which that sagacious chemist lays so much stress, has not latterly, been deemed necessary in acids; and that in bases never was required. As respects them, it only served as a mean of subdivision between alkaline oxides and other oxybases.



I am at a loss to discover in what part of my letter there was any language which could convey the erroneous impression, that, in defining acids and bases I proposed to overlook properties, and to be regulated by attention to the number of atoms in a compound. Certainly nothing was more foreign to my thoughts.

It is assumed by Berzelius that the saturation of the fluobase of potassium by fluohydric acid, cannot be considered as analogous to the saturation of the oxybase of potassium by sulphuric acid; because the resulting compound is to the taste, in one case neutral, in the other sour. In reply I suggested that if the salidity of the biborates and bicarbonates was not to be questioned on account of their alkaline taste, nor that of the protochloride of tin on account of its sourness, it was not consistent that the pretensions to salidity of the fluohydrate of the fluobase of potassium should be denied on account of its sour taste. I will now add that if the fluosilicate of potassium be a double salt, the fluoride of silicon one of its two constituents must be a simple salt, and yet it is sour. If a simple salt may be sour, why may not a double salt have this attribute; and how in fact can its presence be inconsistent with salidity? Is not the absence of this characteristic in silica and tannin, and many other acids, as much against their claims to acidity, as its presence in other compounds is an objection to their association with saline bodies. It is considered by Berzelius an objection to the views which I have espoused, that the halogen bodies, while forming acids with various metallic radicals which oxygen does not acidify, do not form acids with sulphur, phosphorus, and arsenic which oxygen does acidify; yet what is there in this, more difficult to reconcile with the established results of chemical combinations, than in the fact that oxygen forms with sulphur, phosphorus, and arsenic, strong acids, with hydrogen water; while with hydrogen the halogen bodies

all form compounds which Berzelius describes as having the highest pretensions to acidity. The highly active acid properties of the fluorides of boron and silicon, would lead us to expect similar compounds to be formed by the same radicals, with the other halogen bodies, contrary to experience. Chemistry makes us acquainted with many similar discordances. How is it that oxygen forms aeriform compounds with an extremely fixed body in the instance of carbon; while in that of phosphorus or arsenic, both volatilizable, it forms acids which are comparatively insusceptible of volatilization? Wherefore does not hydrogen produce an acid with phosphorus and arsenic, as well as with sulphur?

According to Berzelius, all the halogen bodies produce with hydrogen combinations which are as highly endowed with the attributes of acidity, as the strongest acids into which oxygen enters as a constituent. It is conceded in his letter that his language respecting these combinations cannot be reconciled with his declaration in one place that they do not combine with oxybases and in another that a body which cannot so combine is not an acid. It strikes me, that the only way in which the admitted inconsistency of his description of these bodies, with his definition of acidity, can be avoided, is by assuming that they combine as acids with haloid bases, although decomposed by oxybases.

I will now proceed to comment on a new subject for consideration, presented in Berzelius's letter in reply to mine.

It must be evident that every oxysalt, composed of an oxacid and an oxybase, must consist of an atom of each radical, and as many atoms of oxygen as exist both in the acid and in the base. Thus sulphate of potash consists of an atom of potassium, an atom of sulphur and four atoms of oxygen, and may be represented either by  $\text{SOOO KO}$  or  $\text{SOOOOK}$ .

Berzelius in his letter repeats an ingenious suggestion previously advanced in his treatise, that  $\text{SOOOO}$  (sulphur



with four atoms of oxygen) may act, as a compound halogen body like cyanogen, and thus form a salt by union with an atom of any radical. He conceives that the apparent want of analogy, which induced him to separate into two classes, the amphigen and halogen bodies, disappears under this view of the phenomena; and that his amphide salts might be considered as constituted of a compound halogen body and an elementary radical. But however we may admire the ingenuity of these suggestions, ere, in obedience to them, we extend the limits of the halogen class, I would request that the word salt should be defined, and that it be shown that consistently with any definition which can be devised, there is any class of bodies in nature which merit the appellation of salt producers. Before enlarging the superstructure, let it be shown that the basement has been well grounded.

Berzelius lays some stress on the community of effect, in the evolution of hydrogen, both by acids formed by hydrogen with halogen bodies, and by diluted hydrous sulphuric acid, as evincing a similitude of composition justifying the suggestion above quoted from him. But I conceive that this common result is better explained by ascribing it to the tendency of radicals to displace each other from combination, whether existing in a simple or a complicated compound. If water exists as a base in hydrous sulphuric acid, as I have elsewhere suggested, we may consider this hydrous acid as a sulphate of the oxybase of hydrogen; and that when it reacts with zinc or iron, the proneness of hydrogen to the aeriform state enables either metal to take its place, agreeably to the established laws of affinity.

It may be proper, before concluding, to explain more particularly the nomenclature which I have adopted.

The amphigen, the halogen bodies of Berzelius as they produce acids and bases according to my definition, are all classed as basacigen bodies. Of course oxygen, chlorine,

bromine, iodine, fluorine, cyanogen, sulphur, selenium and tellurium, are included in this class.

The general designation of a binary compound of a basacigen body, is the termination in *ide*; the special, the termination in *acid*, when the compound acts as an acid, in *base*, when it acts as a base.

Hence an oxide, may be an oxacid, or an oxybase;

a chloride,	a chloracid,	or a chloribase;
a bromide,	a bromacid,	or a bromibase;
an iodide,	an iodacid,	or an iodobase;
a cyanide,	a cyanacid,	or a cyanobase;
a sulphide,	a sulphacid,	or a sulphobase;
a selenide,	a selenacid,	or a selenibase;
a telluride,	a telluracid,	or a telluribase.

Compounds which consist of radicals only, are distinguished by the term *uret* equivalent to the French *ure*. Hence *carburet*, *phosphuret*, *boruret*, *silicuret*, &c.

Of any two binary compounds containing each the same basacigen body and forming one compound, the more electro-negative is an acid, the other a base. Hence all the electro-negative haloid compounds in the Berzelian double salts, are acids, and the electro-positive, bases. Where there are two such compounds one containing one basacigen atom, the other two atoms or one and half, the former has a termination in *ous*, the latter in *ic*. As for instance the *chlorureplatinosopotassique* of Berzelius, is a compound of *chloroplatinous acid*, and the *chlorobase* of potassium, and is the chloroplatinite of potassium. The *chlorureplatinico-potassique* of the same author, is the *chloroplatinate of potassium*.

The terms amphigen and halogen being employed both from expedience, and in honor of their author, we may use his term haloid and amphide, to distinguish the acids or bases severally formed by these classes, the abbreviations *halo* and *amph*, being employed in composition. Thus I designate



the acids formed by the halogen bodies with hydrogen, as halohydric acids; those formed with that radical by the amphigen bodies, as amphydric acids. As the same radical will in other cases be found to form acids with several of the halogen bodies, platinum for instance, the acid thus produced, may be called haloplatinic acids; or if gold were the radical, they would be haloauric acids. These examples will suggest to the chemical reader a series of names, as for instance *haloargentic*, *halocupric*, *halostannic*, *halopalladic*.

I consider prussian blue as a cyanoferrite of the cyanobase of iron, or briefly a cyanoferrite of iron. The diversity of properties which enables two cyanides of iron to exist in combination in this cyanoferrite, one as an acid, the other as a base, is one among many other instances in which compounds constituted of the same elements in the same ratio, have different properties, and are said in consequence to be *isomeric*, or to afford cases of *isomerism*.

The salt designated by Berzelius as the "cyanure ferroso-potassique," is the well known test for iron heretofore called ferropotassiate of potassa; under the idea that it consisted of prussic acid, iron and potassa. As the prussic acid was viewed at the same time as a compound of hydrogen and cyanogen, the ferropotassic acid was considered as a compound of cyanogen, hydrogen, and iron. According to Berzelius, the supposed *ferropotassiate* is a compound of a "protocyanure" of iron, and a "*cyanure of potassium*"; each being a simple haloid salt and the aggregate a double "*cyanure*." Agreeably to my nomenclature, the "*protocyanure*" of iron is considered as cyanoferrous acid, and the "*cyanure*" of potassium as a cyanobase; the aggregate being a cyanoferrite of the cyanobase of potassium, but designated briefly as a cyanoferrite of potassium.

I infer that the "*ferropotassic*" acid is analogous in constitution to the triple compound of fluorine, silicon and

hydrogen, improperly called hydrofluosilicic acid; and that, consistently with the hypothetical views under which the latter received its name, the former should be called hydrocyanoferric acid. Even admitting the correctness of the hypothetical impression, to which I have alluded, agreeably to which such compounds are acids with a double radical, I urged that the appellations of such compounds should be so altered as to give precedence to the electro-negative ingredient. Hence the one would be called cyanohydroferric acid; and the other, fluohydrosilicic acid. But in my letter to Prof. Silliman, already cited, I advanced a new hypothesis respecting the constitution of the fluohydrosilicic, and fluohydroboric acids. I suggested that they should be considered as compounds in which the fluorides of silicon or boron acted as acids, the fluoride of hydrogen as a base. Consistently with that doctrine, I would consider *protocyanide* (or "*cyanure*") of iron in the alleged *ferroprussic acid*, as acting as *cyanoferrous acid*, the *cyanide* of *hydrogen* (*prussic acid*) as a *cyanobase* forming, by their union, a cyanoferrite of hydrogen.

As compounds, consisting of a basacigen body, hydrogen and a radical, do not, when presented to bases, enter into combination; but are on the contrary, decomposed so as to allow another radical to take the place of their hydrogen, it is inconsistent with chemical law, as stated by Berzelius, or my definition of acidity, to designate them as acids.

I have called the electro-negative "*protocyanure*" of iron of Berzelius, cyanoferrous acid, because there is "*sesquicyanure*" in the "*cyanureferrico-potassique*" of that author, which by analogy with the nomenclature of the oxacids, is entitled to the appellation of cyanoferric acid."

A contemporary (1843) in speaking of the preceding brochure said:

"This is a very acute and able discussion of an obscure and difficult subject. . . . An attempt to subvert the



present nomenclature of the oxysalts, and to introduce a new arrangement of their elements, so as to make them correspond with the haloid salts, appears to us very unnecessary, and to be unsupported by any reasons, sufficiently important, to justify so annoying an innovation. Men of acute minds may arrange mentally the chemical atoms so as to produce results which harmonize, and leave no fractions to be disposed of. But bounds should be set to these intellectual recreations, especially when they produce a host of new names for principles whose existence cannot be proved, because they cannot be isolated. Many of the names, for example, recently introduced into the organic chemistry, are uncouth, complex, hypothetical, and at war with euphony. Dr. Hare's argument, as regards the new nomenclature of the oxysalts, appears to us to be conclusive, and we trust that the beautiful language so long in use will not be set aside, nor the still more beautiful harmony of the saline elements disturbed."

In addition there is the subjoined letter addressed to the editors of the *American Journal of Science*. Its contents are very interesting:

"Dear Sirs:

In September, 1833, I published in your Journal, together with some encomiums upon the treatise of Chemistry by the celebrated Berzelius, certain objections to his nomenclature, and some suggestions respecting a substitute, which I deemed to be preferable. In the following June I addressed a letter to Professor Silliman upon the same topics, in which my criticisms and suggestions were amplified and corrected in obedience to more mature reflection. A printed copy of that letter having been sent by me to Berzelius, I received in answer an epistle, of which I furnish you with a translation.

Since the period of that correspondence, so demonstrative

of candor and good feeling on the part of the great Swedish chemist, I have published two editions of my Compendium of Chemistry, in which I have pursued a course corresponding with my criticisms above alluded to. I am therefore desirous, in addition to the letter to Berzelius to lay before the public a recapitulation, a review, and an additional explanation of the grounds upon which I have ventured to employ a language, and an arrangement inconsistent with the practice and opinions of a chemist by whose authority in other respects, I am usually influenced. But before proceeding with the ungracious task of endeavoring to establish the correctness of my views in opposition to those of my friend, I feel that it will be no more than justice to repeat an acknowledgment, already made in my text book, that if De Bonsdorff, myself, and others are right in considering the *double* salts of Berzelius as *simple* salts, it is to the light afforded by his investigations, that we owe the power of seeing the subject correctly. I believe the idea, that any other body besides oxygen could produce both acids and bases capable of forming salts, originated with Berzelius, in the instance of sulphur.

According to the Berzelian nomenclature, bodies which produce salts by a union with radicals are called *halogen* or *salt producing bodies*, while those which with radicals form both acids and bases, capable by their union of constituting salts, are called *amphigen bodies* or *both producers*. Salts, produced by the first mentioned class are called haloid salts; those produced by the other are called amphide salts.

I objected to this classification, that the words *salt*, *acid* and *base*, were broad, vague and unsettled in the acceptation, having, by chemists in general, and especially by Berzelius, been employed to designate substances differing in composition, and extremely discordant in their properties; that no method of defining a salt had been devised, which had not been



founded either on properties or composition, that in the nomenclature of Berzelius properties were disregarded, since among his haloid and amphide salts were found substances, differing extremely in this respect. Thus, for instance, common salt, Glauber's salt, Epsom salt, vitriolated tartar, and cream of tartar, were associated with the fuming liquor of Libavius, the butyraceous chlorides of zinc, antimony, and bismuth, plubum corneum, luna cornea, fluor spar, and the acid fluorides of silicon and boron. I objected also that composition could not be resorted to consistently with his classification; since, agreeably to it, a salt might be either a binary compound of a halogen body with a radical, or consist of two binary compounds, each containing the same amphigen body.

To the terms *acid* and *base*, as employed in his nomenclature, I objected, that neither by the celebrated author, nor by any other chemist had any definition been adhered to which could, consistently with his plan, restrict the meaning of those appellations to be binary compounds formed by the union of his amphigen bodies with radicals.

Acidity and basidity<sup>7</sup> had sometimes been distinguished by an appeal to properties, sometimes to composition, but to neither had there been any consistent attention. In order to demonstrate the total neglect of properties latterly displayed, it was only necessary to contrast substances bearing generally the name of acids; as for instance sulphuric acid with rock crystal, acetic acid with tannin, and prussic acid with margaric; or to contemplate simultaneously the admission of the hydracids formed with the halogen bodies into the class of acids, while alleged incapable of combining with bases, with the exclusion from that class of nitrous acid, upon the plea of the same incapacity.

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<sup>7</sup> For the use of the words basidity and salidity, I have no authority, but conceive that through their analogy with acidity, their meaning is so obvious as to make it expedient to employ them.

In reference to neglect of composition in forming the class of acids, it will be sufficient to advert to the association in that class, of compounds formed with radicals both by the halogen and amphigen bodies; so that the halogen bodies are in one case producers of salts, in the other producers of acids; in one case act as supporters, acidifiers, or electro-negative principles, in another as radicals to the comparatively electro-positive hydrogen, pre-eminently a radical by the definition of that word given in the treatise of the distinguished author of the nomenclature.

After stating my objections to the basis of the Berzelian nomenclature, I proceeded to mention those to which I considered the superstructure as liable.

Having designated the acid compounds of his amphigen class, by prefixing syllables indicating their electro-negative ingredients; having also in some instances, as in those of the fluosilicic and fluoboric acids, adopted this course in relation to halogen bodies; I objected to the use of the word hydracid, in which the electro-positive radical is made to act as if co-ordinate with oxygen.

Moreover, the termination in *ide* having been generally attached to the electro-positive compounds of oxygen, acting as bases, I condemned the employment of that termination, to distinguish the electro-positive compounds and acid compounds of sulphur, selenium, and tellurium. I considered it inconsistent to give precedence to the syllable designating the radical in the acids formed with hydrogen; as in hydrochloric, hydrobromic, hydriodic, hydrofluoric, hydrofluoboric, hydrofluosilicic, preferring the terms chlorohydric, bromohydric, iodhydric, fluohydroboric, fluohydrosilicic, &c., in which I have been sanctioned by Thenard and others.

I proposed a definition of an acid and a base, which I conceived to be the only one which could be adopted, consistently with the uses made of those words by Berzelius, and other



distinguished chemists; and advanced that, agreeable to that definition, his double haloid salts must be considered as *simple* salts, severally formed of an acid and a base.

I objected to his treating the words combustion and oxygenation as synonymous."

To slightly anticipate chronologically, the following elaborate presentation of Hare's ideas on radicals may now find place. The communication is very closely attached to his previously expressed views. It bears the title:

An effort to refute the arguments advanced in favor of the Existence, in the Amphide Salts, of Radicals consisting, like Cyanogen, of more than one element, and reads as follows:

The following is a summary of the opinions, which it is the object of the subsequent reasoning to justify.

a. The community of effect, as respects the extrication of hydrogen by contact of certain metals with aqueous solutions of sulphuric and chlorohydric acid, is not an adequate ground for an inferred analogy of composition, since it must inevitably arise that any radical will, from any compound, displace any other radical, when the forces favoring its substitution, preponderate over the quiescent affinities.

b. But if, nevertheless, it be held that the evolution of hydrogen from any combination, by contact with a metal, is a sufficient proof of the existence of a halogen body, simple or compound, in the combination, the evolution of hydrogen from water, by the contact with any metal of the alkalies, must prove oxygen to be a halogen body, also the evolution of hydrogen from sulphydric, selenhydric, or telluhydric acids, by similar means, would justify an inference that sulphur, selenium, or tellurium, as well as oxygen, belong to the halogen, or "salt radical" class.

c. The amphigen bodies being thus proved to belong to the halogen class, oxides, sulphides, selenides, and tellurides,

would be haloid salts, and their compounds double salts, instead of consisting of a compound radical and a metal.

d. The argument in favor of similarity of composition in the haloid and amphide salts, founded on a limited resemblance of properties in some instances, is more than counterbalanced by the extreme dissimilitude in many others.

e. As, in either class, almost every property may be found which is observed in any chemical compound, the existence of a similitude, in some cases, might be naturally expected.

f. As it is evident that many salts, perfectly analogous in composition, are extremely dissimilar in properties, it is not reasonable to consider resemblance in properties, as a proof of analogy in composition.

g. No line of distinction, as respects either properties or composition, can be drawn between the binary compounds of the amphigen and halogen bodies, which justifies that separate classification which the doctrine requires; so that it must be untenable as respects the one, or be extended to the other.

h. The great diversity, both as respects properties and composition, of the bodies called salts, rendering it impossible to define the meaning of the word, any attempt to vary the language and theory of chemistry, in reference to the idea of a salt, must be disadvantageous.

i. There is at least as much mystery in the fact, that the addition of an atom of oxygen to an oxacid, should confer an affinity for a simple radical, as that the addition of an atom of this element to such a radical, should create an affinity between it, and an oxacid.

j. If one atom of oxygen confer upon the base into which it enters, the power to combine with one atom of acid, it is quite consistent that the affinity should be augmented, proportionably, by a further accession of oxygen.

k. It were quite as anomalous, mysterious, and improbable, that there should be three oxyphosphions, severally



requiring for saturation one, two, and three atoms of hydrogen, as that three isomeric states of phosphoric acid should exist, requiring as many different equivalents of basic water.

l. The attributes of acidity alleged to be due altogether to the presence of basic water, are not seen in hydrated acids, when holding water in that form only; nor in such as are, like the oily acids, incapable of uniting with water as a solvent. Further, these attributes are admitted to belong to salts which, not holding water as a base, cannot be hydrurets or hydracids of any salt radical; and while such attributes are found in compounds which, like chromic, or carbonic acid, cannot be considered as hydrurets, they do not exist in all that merit this appellation, as is evident in the case of prussic acid, or oil of bitter almonds.

m. It seems to have escaped attention, that if  $\text{SO}_4$  be the oxysulphion of sulphates,  $\text{SO}_3$  anhydrous sulphuric acid, must be the oxysulphion of the sulphites; and that there must, in the hyposulphites and hyposulphates, be two other oxysulphions.

n. The electrolytic experiments of Daniell have been erroneously interpreted, since the electrolysis of the base of sulphate of soda would so cause the separation of sodium and oxygen, that the oxygen would be attracted to the anode, the hydrogen and soda being *indirectly* evolved by the reaction of sodium with water; while the acid, deprived of its alkaline base, would be found at the anode in combination with basic water, without having been made to act in the capacity of an anion.

o. The copper in the case of a solution of the sulphate of this metal and a solution of potash, separated by a membrane, would, by electrolyzation, be evolved by the same process as sodium, so long as there be copper to perform the office of a cathion; and when there should no longer be any copper to act in this capacity, the metal of the alkali, or hydro-

gen of water, on the other side of the membrane, would act as a cation; the oxygen acting as an anion from one electrode to the other, first to the copper, and then to the potassium.

p. The allegation that the copper was deposited from the want of an anion (oxysulphion) to combine with, is manifestly an error, since, had there been an anion, there could have been no discharge, as alleged, to hydrogen as a cation, nor any electrolysis.

q. The hydrated oxide precipitated on the membrane, came from the reaction of the alkali with the sulphate of copper; the precipitated oxide of this metal from the oxygen of the soda action as an anion; and the deposit of metallic copper from the solutions performing, feebly, the part of electrodes, while themselves the subjects of electrolyzation.

r. The so called principles of Liebig, by which his theory of organic acids is preceded, are mainly an inversion of the truth, since they make the capacity of saturation of hydrated acids dependent on the quantity of hydrogen in their basic water, instead of making both the quantity of water, and, of course, the quantity of hydrogen therein, depend on their capacity.

s. All that is truly said of hydrogen, would be equally true of any other radical, while the language employed would lead the student to suppose that there is a peculiar association between capacity of saturation, and presence of hydrogen.

1. Some of the most distinguished European chemists, encouraged by the number of instances in which the existence of hypothetical radicals has been rendered probable, have lately inferred the existence of a large number of such radicals in a most important class of bodies, heretofore considered as compounds of acids and bases. It has been inferred, for instance, that sulphur, with four atoms of oxygen ( $\text{SO}_4$ ) constitutes a compound radical, which performs in hydrous sulphuric acid, the same part as chlorine in chlorohydric acid.



2. Graham has proposed sulphatoxygen as a name for this radical, and sulphatoxide for any of its compounds. Daniell has proposed oxysulphion and oxysulphionide for the same purposes. In reasoning on the subject I shall use the nomenclature last mentioned, not, however, with a view to sanction it, as I disapprove altogether of this innovation, and deny the sufficiency of the grounds upon which it has been justified. Consistently with the language suggested by Daniell, hydrous sulphuric acid, constituted of one atom of acid and one of basic water ( $\text{SO}_3 + \text{HO}$ ), is a compound of oxysulphion and hydrogen ( $\text{SO}_4 + \text{H}$ ). Nitric acid ( $\text{NO}_5 + \text{H}$ ) is a compound of oxynitron and hydrogen ( $\text{NO}_6 + \text{H}_5$ ). In like manner we should have oxyphosphion in phosphoric acid, oxyarsenion in arsenic acid, and in all acids, hitherto called hydrated, whether organic or inorganic, we should have radicals designated by names made after the same plan. Their salts having corresponding appellations, would be oxysulphionides, oxynitronides, &c. Also, in any salt in which any other of the amphigen class of Berzelius is the electro-negative ingredient, whether sulphur, selenium, or tellurium, all the ingredients excepting the electro-positive radical, would be considered as constituting a compound electro-negative radical.

3. It may be expedient to take this opportunity of mentioning that the advocates of this new view, disadvantageously, as I think, employ the word radical, to designate the electro-positive ingredient. Agreeably to the nomenclature of Berzelius, the former would be a compound halogen body. Cyanogen being analogous, is by him placed in the halogen class. I shall, therefore, in speaking of "*salt radicals*," improperly so called, employ the appellation contrived by the great Swedish chemist.

4. Nevertheless it seems to be conceded, that however plausible may be the reasons for inferring the existence of

halogen bodies in the amphide salts, it would be inexpedient to make a corresponding change in nomenclature, on account of the great inconvenience which must arise from the consequent change of names.

5. Under these circumstances, it may be well to consider how far there is any necessity for adopting hypothetical views, to which it would be so disadvantageous to accommodate the received language of chemists. In the strictures on the Berzelian nomenclature, which drew from Berzelius the suggestions previously given, I state it to be my impression that water should be considered as acting in some cases as an oxybase, in others as an oxacid; and, in my examination of his reply, I observed that *hydrous sulphuric acid might be considered as a sulphate of hydrogen, and that when this acid reacts with zinc or iron, the proneness of hydrogen to the aeriform state enables either metal to take its place, agreeably to the established laws of affinity.*

6. There appears to have been a coincidence of opinion between Kane, Graham, Gregory, and myself, as respects the electro-positive relation of hydrogen to the amphigen and halogen elements, which I have designated collectively as the basacigen class; also in the impression that hydrogen acts like a metallic radical, its oxide, water, performing the part of a base. I agree perfectly with Gregory in considering that hydrated acids may be considered as "hydrogen salts." But when the learned editor proceeds to allege that "acids and salts, as respects their constitution, will form one class," I consider him, and those who sanction this allegation, as founding an error upon an oversight. Because the salts of hydrogen, or such as have water for their base, have heretofore been erroneously called acids, we are henceforth to confound salts with acids, and, instead of correcting one wrong name, cause all others to conform thereto!

7. I fully concur with Gregory and Kane, in considering



that water in hydrous sulphuric acid, in nitric acid, chloric acid, and in organic acids, generally acts as a base; also, that in this basic water hydrogen performs a part perfectly analogous to that of a metallic radical; but, agreeably in accounting for the evolution of hydrogen, as suggested in the quotation above made (6), agreeably to which, when diluted sulphuric acid reacts with zinc, or iron, the liberation of hydrogen results from the superiority of the forces which tend to insert either of these metals in the places occupied by the hydrogen, over those which tend to retain it *in statu quo*.

8. When oxide of copper is presented to chlorohydric acid, it is inferred that the hydrogen unites with oxygen, and the chlorine with the metal; and hence it seems to be presumed, that when oxide of copper is combined with sulphuric acid, a similar play of affinities should ensue; but would it be reasonable to make this a ground for assuming the existence of a compound radical, when the phenomena admit of another explanation quite as simple and consistent with the laws of chemical affinity?

9. Whether hydrogen be replaced by zinc, or oxide of hydrogen by oxide of copper, cannot make any material difference. In the one case, a radical expels another radical, and takes its place; in the other, a base expels another base, and takes its place.

10. There can be no difficulty, then in understanding wherefore, from the compound of sulphur and three atoms of oxygen, and an atom of basic water, hydrogen should be expelled and replaced by zinc, or that water should be expelled and replaced by oxide of copper; the only mystery is in the fact, that  $\text{SO}_3$  as anhydrous sulphuric acid, will not combine with hydrogen, copper, or any other radical, unless oxidized. But this mystery equally exists on assuming that an additional atom of oxygen converts  $\text{SO}_3$  into oxysulphion, endowed with an energetic affinity for metallic radicals, to which  $\text{SO}_3$  is quite indifferent.

11. In either case, an inexplicable mystery exists; but it is, in the one case, associated with an hypothetical change, in the other, with one which is known to take place.

12. But if hydrous sulphuric acid is to be assumed to be a hydruret of a compound halogen body (oxysulphion), because it evolved hydrogen on contact with zinc, wherefore is not water, which evolved hydrogen on contact with potassium, sodium, barium, strontium, or calcium, to be considered as a hydruret of oxygen, making oxygen a halogen body?

13. Boldly begging the question, Graham reasons thus: "*The chlorides themselves being salts, their compounds must be double salts.*"

14. But if the chlorides are salts, the chloride of hydrogen is a salt; and if so, wherefore is not the oxide of hydrogen a salt, which, in its susceptibility of the crystalline form, has a salt attribute which the aeriform chloride does not possess?

15. Further, if the oxide of hydrogen be a salt, every oxide is a salt, as well as every chloride. Now, controverting the argument above quoted, by analogous reasoning, it may be said, "*the oxides themselves being salts, their compounds are double salts.*" Of course sulphate of potash is not a sulphatoxide, as Graham's ingenious nomenclature would make it, but must be a double salt, since it consists of two oxides in "themselves salts."

16. I trust that sufficient reasons have been adduced, to make it evident that the common result of the extrication of hydrogen, during the reaction of zinc or iron with sulphuric or chlorohydric acid, if not a competent ground for assuming that there are, in amphide salts, "compound radicals" playing the same part as halogen bodies.

17. Let us, in the next place, consider the argument in favor of the existence of such radicals, founded on the similitude of the haloid and amphide salts, which is stated by Kane in the following words:—



“It had long been remarked as curious, that bodies so different in composition as the compound of chlorine with a metal, on one hand, and of an oxygen acid with the oxide of the metal on the other, should be so similar in properties, that both must be classed as salts, and should give rise to a series of basic and acid compounds, for the most part completely parallel.” *Elements* p. 681.

18. Upon the *similitude* and *complete parallelism* of the amphide and haloid salts, thus erroneously alleged, the author proceeds to argue in favor of the existence in the former, of compound halogen bodies, analogous in their mode of combination to chlorine or iodine.

19. I presume it will be granted, that if similitude in properties be a sufficient ground for inferring an analogy in composition, dissimilitude ought to justify an opposite inference. And that if, as the author alleges, certain bodies have been classed as salts, on account of their similarity in this respect, when dissimilar they ought not to be so classed. Under this view of the question, I propose to examine how far any similitude in properties exists between the bodies designated as salts by the author, or any other chemist.

20. The salts, hitherto considered as compounds of acids and bases, are by Berzelius called amphide salts, being produced severally by the union with one or the other of his amphigen class, comprising oxygen, sulphur, selenium, and tellurium, with two radicals, with one of which an acid is formed, with the other a base. The binary compounds of his halogen class, comprising chlorine, bromine, iodine, fluorine and cyanogen, are called by him haloid salts. I shall use the names thus suggested.

21. Among the haloid salts we have common salt and Derbyshire spar; the gaseous fluorides and chlorides of hydrogen, silicon or boron; the fuming liquor of Libavius; the acrid butyraceous chlorides of zinc, bismuth, and antimony; the vol-

atile chlorides of magnesium, iron, chromium, and mercury, and the fixed chlorides of calcium, barium, strontium, silver, and lead; the volatile poison prussic acid, and solid poison bicyanide of mercury, with various inert cyanides like those of Prussian blue; likewise a great number of ethereal compounds.

22. Among the amphide salts are the very soluble sulphates of zinc, iron, copper, soda, magnesia, &c., and the insoluble stony sulphates of baryta and strontia; also ceruse and sugar of lead; alabaster, marble, soaps, ethers, and innumerable stony silicates and aluminates. Last, but not among the least discordant, are the hydrated acids, and alkaline and earthy hydrates.

23. When the various sets of bodies, above enumerated, as comprised in the two classes under consideration, are contemplated, is it not evident that, not only between several sets of haloid and amphide salts, but also between several sets in either class, there is an extreme discordancy in properties; so that making properties the test, would involve not only that various sets in one class could not be coupled with certain sets in the other, but, also, that in neither class could any one set be selected as exemplifying the characteristics of a salt, without depriving a majority of those similarly constituted, of all pretensions to the saline character?

24. Now, if among the bodies above enumerated, some pairs of amphide and haloid salts can be selected, which make a tolerable match with respect to their properties, as in the case of sulphate of soda, and chloride of sodium, while in other cases there is the greatest discordancy, (as in the stony silicate felspar, and the gaseous fluoride fluosilicic acid gas; as in soap and Derbyshire spar; as in marble and the fuming liquor of Libavius, the sour protochloride of tin, and sweet acetate of lead), is it reasonable to found an argument in favor of a hypothetical similitude in composition, on the *resemblance* of the two classes in properties? Does not the



*extreme* dissimilitude in some cases, more than countervail the limited resemblance in others? And when the great variety of properties displayed both by the amphide and haloid salts is considered, is it a cause for wonder or perplexity, that in some instances, amphide salts should be found to resemble those of the other kind?

25. Again, admitting that there was any cause for perplexity agreeably to the old doctrine, is there less, agreeably to that which is now recommended? Is there no ground for wonder that oxygen or sulphur cannot act as simple halogen bodies? By what rule are their binary compounds to be excluded from the class of haloid salts? Wherefore should chlorides, bromides, iodides, and fluorides, however antisaline in their properties, be considered as salts, while in no case is an oxide, a sulphide, selenide or telluride to be deemed worthy of that name?

26. I challenge any chemist to assign any good reason wherefore the red iodide of mercury is any more a salt than the red oxide, or the protochloride is more saline than the sulphide; or why the volatile oxides of osmium or of arsenic are less saline than horn silver or horn lead; or the volatile chloride of arsenic, than the comparatively fixed sulphides of the same metal; why gaseous chlorohydric acid is more saline than steam or gaseous oxhydric acid.

27. It much surprises me, that when so much stress is laid upon the idea of a salt, the impossibility of defining the meaning of the word escapes attention. How is a salt to be distinguished from any other binary compound? When the discordant group of substances which have been enumerated under this name is contemplated, is it not evident that no definition of them can be founded on community of properties? and, by the advocates of the new doctrine, composition has been made the object of definition, instead of being the basis; thus, agreeably to them, a compound is not a salt, be-

cause it is made of certain elements; but, on the contrary, an element, whether simple or compound, belongs to the class of salt radicals, because it produces a salt. Since sulphur, with four atoms of oxygen,  $\text{SO}_4$ , produces a salt with a metal, it must be deemed a salt radical.

28. In proof that the double chlorides are not united in a way to justify the opinion adopted by Bonsdorff, Thomson, myself, and others, it is alleged by Graham, "that in such compounds the characters of the constituent salts are very little affected by their state of union."

29. This allegation being, in the next page, admitted to be inapplicable in the case of the double cyanides; an effort is made to get over this obstacle, by suggesting the existence of another compound radical. But the allegation of the author is erroneous as respects various double haloid salts, especially the fluosilicates, the fluoborates, fluozirconiates, the chloroplatinates, chloriridates, chloroosmiate, chloropalladiates, &c., all of them compounds in which the constituent fluorides and chlorides exist in a state of energetic combination, by which they are materially altered as to their state of existence.

30. Evidently the word salt has been so used, or rather so abused, that it is impossible to define it, either by a resort to properties or composition; and I conceive, therefore, that to make it a ground of abandoning terms which are susceptible of definition, and which have long been tacitly used by chemists in general, in obedience to such definition, would be a "*retrograde movement in the science.*" I hope Dr. Kane will pardon me for employing the language to which he has resorted, in speaking of the opinions of Bonsdorff.

31. If this doctrine, as it has been stated, is to prevail, I do not perceive how it is to be prevented from claiming an inconvenient extension. The hydrates, as well as the sulphates, must have pretensions to contain salt radicals. Hence in the hydrated alkalies and alkaline earths, there would be



a compound radical, consisting of hydrogen, with two atoms of oxygen, hydroxion, and these compounds would be hydroxionides; nor can I conceive that the haloid compounds, erroneously called double salts, but more correctly considered as single salts, can be exempted.

32. Between the reaction of fluoboric acid with fluobases, and sulphuric acid with oxybases, is there not a great resemblance?

33. I am unable to understand how, if the existence of salt radicals in oxysalts be inferred, the other salts of the amphigen class can be exempted from a corresponding inference. But if the existence of salt radicals in the double sulphides be admitted can it be consistently denied that they exist also in double chlorides, iodides, &c.? Is there not the greatest analogy between the habitudes of sulphur, selenium, and tellurium, with metals, and those of the halogen bodies?

34. Would not the modification of the ethereal oxysalts, to comport with the new hypothesis, be disadvantageous, both as respects our mental conception of those compounds, and the names which would be rendered appropriate? Would not the transfer of the oxygen from the ethereal oxide to the acid, and the creation, thus, of new salt radicals for the organic acid salts, be objectionable; such as oxyoxalion for oxalates, oxytartarion for tartrates, oxyaceton for acetates; while for their compounds, we should have oxyoxalionides, oxytartarionides, oxyacetonides, &c.?

35. If sulphates are to be considered as oxysulphonides, by what names are we to designate the sulphites, hyposulphites, and hyposulphates,  $\text{SO}_2$ ,  $\text{S}_2\text{O}_2$ ,  $\text{S}_2\text{O}_5$ ?  $\text{SO}_3$  may, perhaps, with more propriety be considered as consisting of a compound radical,  $\text{SO}_2$ , and oxygen, forming an oxide of sulphurous acid; but in a sulphite, anhydrous sulphuric acid,  $\text{SO}_3$  becomes a species of oxysulphion itself, being as much the oxysulphion of the sulphites, as  $\text{SO}_4$  is of the sulphates.

Of course  $\text{SO}_3$  should have a direct affinity for radicals, contrary to fact. I presume that sulphites would have to be trioxysulphonides; hyposulphites, sesquioxysulphonides; sulphates, quadroxysulphonides; while the hyposulphates would, I suppose, be demiquintoxysulphonides!!!

36. Analogous complication in nomenclature would arise in respect to the nitrites and nitrates, phosphites and phosphates, arsenites and arseniates; also as respects the carbonic and oxalic acids.

37. It is true that nature has not so made her bodies as that they can be separated into classes, between which any distinct line can be drawn, still it has been found advantageous to classify them to the best of our power. Accordingly it appears to me expedient, in the first place, to distinguish elements (or those compounds which act like them) according to their electro-chemical relations to each other, or their habitudes with the voltaic electrodes. Consistently, chemists have tacitly adopted the plan of treating the compounds formed by electro-negative elements with anions, as acids; those formed with cations, as bases; while the combinations formed by the union of such acids and bases have been considered as simple salts. Thus four classes are constituted, consisting of electro-negative elements, of acids, bases, and single salts, while, by the union of the latter, a fifth class of double salts is formed. Whether the words acid, base, and salt, be adhered to, objectionable as they are in some respects, and especially the latter, or some others be contrived, it would seem to me disadvantageous to merge them in one name, pursuant to the views of the advocates of salt radicals, as stated by Gregory in his edition of Turner's Chemistry, 572.

38. The objection, that not being electrolytes the relation of acids and bases to the voltaic electrodes cannot be discovered, is easily remedied; since, on the union of a common ingredient with an anion and a cation, there cannot be any



doubt that the resulting compounds will have the same electro-chemical relation as their respective heterogeneous ingredients; so that, with the anion, an acid or electro-negative body will be formed; with the cation, a base or electro-positive body. Moreover, as respects organic compounds which cannot be subjected to the electrolytic test, whatever saturates an inorganic acid must be a base, and whatever saturates an inorganic base must be an acid.

39. The word salt, I have shown, is almost destitute of utility, from the impossibility of defining it, and the amplitude of its meaning. A word that means everything, is nearly as useless as that which means nothing.

40. As respects the three phosphates of water,  $\text{PO}_5 + \text{HO}$ ,  $\text{PO}_5 + 2\text{HO}$ ,  $\text{PO}_5 + 3\text{HO}$ , the argument used by Dr. Kane cuts both ways; although, by its employer, only that edge is noticed which suits his own purpose. It is alleged that the difference of properties, in these phosphates, is totally inexplicable upon the idea of three degrees of "hydration"; but that all difficulty vanishes, when they are considered as three different compound salt radicals, oxyphosphonides of hydrogen  $\text{PO}_6 + \text{H}$ ,  $\text{PO}_7 + 2\text{H}$ ,  $\text{PO}_8 + 3\text{H}$ .

41. To me the formation of three compound elements, by the reiterated addition of an atom, of which five of the same kind were previously in the mass to which the addition is made, seems more anomalous, mysterious, and improbable, than the existence of three compounds of phosphoric acid with water, in which the presence of the different proportions of water is the consequence of some change in the constitution of the elements, which is referred to isomerism.

42. No reason can be given why the addition of *one*, *two* and *three* atoms of oxygen, to the "radical," should convey a power to hold a proportional number of atoms of hydrogen. Such an acquisition of power is an anomaly.

43. In the case of radicals formed with hydrogen in

different proportions, as in acetyl and ethyl, formyl and methyl, the number of atoms of oxygen in the peroxides, is the inverse of the hydrogen in the radical.

44. Ethyl,  $C_4H_4$ , unites, at most with one atom of oxygen, while acetyl,  $C_4H_3$ , takes three atoms to form acetic acid,  $C_4H_3O_3$ . Methyl,  $C_2H_3$ , forms, in like manner, only a protoxide, while formyl,  $C_2H$ , takes three atoms to constitute formic acid.

45. Besides the three oxyphosphions, of which the formulas are above stated, there would have to be another in the phosphites; so that instead of the hydrated acid, or phosphite of water, being  $PO_3 + HO$ , it would have to be  $PO_4 + H$ , a fourth oxyphosphionide of hydrogen.

46. Respecting the new principles which I have been contesting, Dr. Kane, alleges "that the elegance and simplicity with which the laws of saline combination may be traced from them is remarkable," because he conceives, that without an appeal to those principles, the fact that the number of equivalents of acid in a salt are portionable to the number of equivalents of oxygen in the base, would be inexplicable.

47. Thus, when the base is a protoxide, we have one atom of the protoxide of hydrogen to take its place; when the base is a sesquioxide (two of radical and three of oxygen), three atoms of the protoxide of hydrogen take its place: if the base be a bioxide, two atoms of the protoxide of hydrogen take its place.

48. I have already adverted to the existence of certain chemical laws, inexplicable in the present state of human knowledge. Among these is that of the necessity of oxidation to enable metallic radicals to combine with acids. But as a similar mystery exists as respects the adventitious property of combining with radicals, which results from the acquisition of an additional atom of oxygen by any of the compounds hitherto considered as anhydrous acids, the new doctrine has in that respect no pre-eminent claim to credence.



49. But if, without impairing the comparative pretensions of the prevailing doctrine, we may appeal to the fact that the acquisition of an atom of oxygen confers upon a radical the basic power to hold one atom of acid, is it not consistent that the acquisition of two atoms of oxygen should confer the power to hold two atoms of acid, and that with each further acquisition of oxygen a further power to hold acids should be conferred?

50. So far then there is in the old doctrine no more inscrutability than in that which has been proposed as its successor. Since if on the one hand it be requisite that for each atom of oxygen in the base, there shall be an atom of acid in any salt which it may form, on the other, in the case of the three oxyphosphions, for each additional atom of hydrogen extraneous to the salt radical, there must be an atom of oxygen superadded to this radical.

51. It being then admitted that, numerically, the atoms of acid in any oxysalt will be as the atoms of oxygen in the base, it must be evident that whenever an oxysalt of a protoxide is decomposed by a bioxide, there will have to be two atoms of the former for one of the latter. For the bioxide has two atoms of oxygen, and requires by the premises two atoms of acid, while the salt of the protoxide, having but one atom of oxygen, can hold, and yield, only one atom of acid. Two atoms, of this salt, therefore, whether its base be water, or any other protoxide, will be decomposed by one atom of bioxide; provided the affinity of the acid for the bioxide predominate over that entertained for the protoxide, as when water is the base.

52. It follows, that the displacement of water from its sulphate, adduced by Kane, does not favor the idea that hydrous sulphuric acid is an oxysulphionide of hydrogen, more than the impression that it is a sulphate of water.

53. Of course, in the case of presenting either a sesquioxide,

or a trioxide, to the last mentioned sulphate, in other words, hydrous sulphuric acid, the same rationale will be applicable.

54. The next argument advanced by Dr. Kane, is, that some of the acids of which the existence is assumed upon the old doctrine, are hypothetical, as they have never been isolated. This mode of reasoning may be made to react against the new doctrine with pre-eminent force, since all of the compound radicals imagined by it are hypothetical—none of them having been isolated.

55. The third argument of the respectable author above named is, that acids display their acid character in a high degree only when in the combination with water.

56. This argument should be considered in reference to two different cases, in one of which all the water held by the acid is in the state of a base, while in the other an additional quantity is present acting as a solvent. So far as water, acting as a solvent, facilitates the reaction between acids and bases, it performs a part in common with alcohol, ether, volatile oils, resins, vitrifiable fluxes, and caloric. Its efficacy must be referred to the general law, that fluidity is necessary to chemical reaction. "*Corpora non agunt nisi soluta.*"

57. In a majority of cases, basic water, unaided by an additional portion acting as a solvent, is quite incompetent to produce reaction between acids and other bodies. Neither between sulphuric acid and zinc, between nitric acid and silver, nor between glacial or crystallized acids and metallic oxides, does any reaction take place without the aid of water acting as a solvent, performing a part analogous to that which heat performs in promoting the union of those oxybases with boric, or silicic acid.

58. It is only with *soluble* acids that water has any efficacy. The difference between the energy of sulphuric and silicic acid, under the different circumstances in which they can reciprocally displace each other, is founded on the nature of



the solvents which they require, the one being only capable of liquefaction by water, the other by caloric.

59. In support of his opinions the author adverts to the fact, that with hydrated sulphuric acid, baryta will combine energetically *in the cold*, while a similar union between the anhydrous vapor and the same base cannot be accomplished *without heat*. But it ought to be recollected, that to make this argument good, it should be shown wherefore heat causes the baryta, a perfectly fixed body, to unite more readily with an aeriform substance in which increase of temperature must, by rarefaction, diminish the number of its particles in contact with the solid. If the only answer be, that heat effects some mysterious changes in affinity, (or as I would say, in the electrical state of the particles,) it should be shown that the presence of water or any other base has not been productive of a similar change, before another explanation is held to be necessary. But I would also call to mind that the hydrated acid is presented in the liquid state; and if it be asked why water, having less affinity than baryta, can better cause the condensation of the acid, I reply, that it is brought into contact with the acid both as a liquid and a vapor, of neither of which forms is the earthy base susceptible. But if all that is necessary to convert anhydrous sulphuric acid into an oxy-sulphonide, be an atom of oxygen and an atom of metal, what is to prevent baryta and anhydrous sulphuric acid from forming an oxysulphonide of barium? All the elements are present which are necessary to form either a sulphate or oxy-sulphonide; and I am unable to conceive wherefore the inability to combine does not operate as much against the existence of radicals as of bases.

60. I would be glad to learn why, agreeably to the salt radical theory, anhydrous sulphuric acid unites with water more greedily than with baryta, and yet abandons the water promptly on being presented to this base. Why should it

form an oxysulphionide with hydrogen more readily than with barium, and yet display, subsequently, a vastly superior affinity for barium?

61. It seems to be overlooked, that anhydrous sulphuric acid, being the oxysulphion of the sulphites, ought to form *sulphites* on contact with metals.

62. But if the sulphate of water owe its energy to that portion of this liquid, which, by its decomposition gives rise to the compound radical oxysulphion, and not to the portion which operates as a solvent, therefore in the concentrated state, will it not react with iron and zinc, without additional water, when, with dilution, it reacts most powerfully with those metals?

63. Some stress has been laid upon the fact, that sourness is not perceived, excepting with the aid of water, as if to derive force for the new doctrine from that old and popular, though now abandoned, test of acidity; but it should be recollected that it is not the water, which goes to form the compound element in the "hydracids," erroneously so called, which confers sourness. Will any one pretend that either sulphuric or nitric acid, when concentrated, is sour? Are they not caustic? Can any of the crystallized organic acids be said to have a sour taste, independently of the moisture of the tongue? The hydrated oily acids being incapable of uniting with water as a solvent, have none of these vulgar attributes of acidity. The absence of these attributes in prussic acid would alone be sufficient to render it inconsistent to consider them as having any connexion with the presence of hydrogen.

64. It has been remarked, that liquid carbonic acid does not combine with oxides on contact. To this I would add, that it does not combine with water under those circumstances, but, on the contrary, separates from it like oil, after mechanical mixture; nor does it, under any circumstances, unite with an equivalent proportion of water to form a hydrate. Of course, as it is not to basic water that it is indebted



for its ability to become an ingredient in salts, it cannot be held that this faculty is the result of its previous conversion into an *oxycarbonide of hydrogen*.

65. Chromic acid is admitted not to require water for isolation, and cannot, therefore, be considered as oxychromionide of hydrogen. Yet the oil of bitter almonds, which consists of a *compound radical*, benzule, and an atom of hydrogen, and which is therefore constituted precisely as the salt radical doctrine requires for endowment with the attributes of an "hydracid," is utterly destitute of that acid reaction which hydrogen is represented as peculiarly competent to impart. It follows that we have, on the one hand, in chromic acid, a compound endowed with the attributes of acidity, without being a hydruret of any compound radical; and, on the other, in oil of bitter almonds, a hydruret of a compound radical, without any of the attributes of acidity.

66. The last argument in favor of the existence of salt radicals, which I have to answer, is that founded on certain results of the electrolysis of saline solutions.

67. On subjecting a solution of sulphate of soda to electrolysis, so as to be exposed to the current employed, simultaneously with some water in a voltameter, Daniell alleges that, for each equivalent of the gaseous elements of water evolved in the voltameter, there was evolved at the cathode and anode, not only a like quantity of those elements, but likewise an equal number of equivalents of soda and sulphuric acid. This he considers as involving the necessity, agreeably to the old doctrine, of the simultaneous decomposition of two electrolytic atoms, in the solution, for one in the voltameter; while, if the solution be considered as holding oxysulphonide of sodium, instead of sulphate of soda, the result may be explained consistently with the law ascertained by Faraday. In that case, oxysulphion would be carried to the anode, where combining with hydrogen, it would cause

oxygen to be extricated, while sodium, carried to the cathode, deoxidizing water, would cause the extrication of hydrogen.

68. Dr. Kane, alluding to the experiments above mentioned, and some others which I shall mention, alleges that "*Professor Daniell considers the binary theory of salts to be fully established by them.*"

69. Notwithstanding the deference which I have for the distinguished inventor of the constant battery, and disinclination for the unpleasant task of striving to prove a friend to be in the wrong, being of opinion that these inferences are erroneous, I feel it to be my duty, as a teacher of the science, to show that they are founded upon a misinterpretation of the facts appealed to for their justification.

70. It appears to me, that the simultaneous appearance of the elements of water, and of acid and alkali, at the electrodes, as above stated, may be accounted for, simply by that electrolyzation of the soda, which must be the natural consequence of the exposure of the sulphate of that base in the circuit. I will in support of the exposition which I am about to make, quote the language of Professor Daniell, in his late work, entitled, "Introduction to Chemical Philosophy," page 413:

"Thus we may conceive that the force of affinity receives an impulse which enables the hydrogen of the first particle of water, which undergoes decomposition, to combine momentarily with the oxygen of the next particle in succession; the hydrogen of this again, with the oxygen of the next; and so on till the last particle of hydrogen communicates its impulse to the platinum, and escapes in its own elastic form."

71. The process here represented as taking place in the instance of the oxide of hydrogen, takes place, of course, in that of any other electrolyte.

72. It is well known, that when a fixed alkaline solution is subjected to the voltaic current, that the alkali, whether



soda or potassa, is decomposed; so that if mercury be used for the cathode, the nascent metal, being protected by uniting therewith, an amalgam is formed. If the cathode be of platinum, the metal, being unprotected, is, by decomposing water, reconverted into an oxide as soon as evolved. This shows, that when a salt of potassa or soda is subjected to the voltaic current, it is the alkali which is the primary object of attack, the decomposition of the water being a secondary result.

73. If in a row of the atoms of soda, extending from one electrode to the other, while forming the base of a sulphate, a series of electrolytic decompositions be induced from the cathode on the right, to the anode on the left, by which each atom of sodium in the row will be transferred from the atom of acid with which it was previously combined, to that next upon the right, causing an atom of the metal to be liberated at the cathode; this atom, deoxidizing water, will account for the soda and hydrogen at the cathode. Meanwhile the atom of sulphate on the left, which has been deprived of its sodium, must simultaneously have yielded to the anode the oxygen by which this metal was oxidized. Of course the acid is left in the hydrous state, usually called free, though more correctly esteemed to be that of a sulphate of water.

74. I cannot conceive how any other result could be expected from the electrolysis of the base of sulphate of soda, than that which is here described. Should any additional illustration be requisite, it will be found in a note subjoined.

75. I will, in the next place, consider the phenomena observed by Professor Daniell, when solutions of potassa and sulphate of copper, separated by a membrane, were made the medium of a voltaic current.

76. Of these I here quote his own account (*Philosophical Magazine and Journal*, vol. xvii, p. 172) :

“A small glass bell, with an aperture at top, had its mouth closed by tying a piece of thin membrane over it. It was

half filled with a dilute solution of caustic potassa, and suspended in a glass vessel containing a strong neutral solution of sulphate of copper, below the surface of which it just dipped. A platinum electrode, connected with the last zinc rod of a large constant battery of twenty cells, was placed in the solution of potassa; and another, connected with the copper of the first cell, was placed in the sulphate of copper immediately under the diaphragm which separated the two solutions. The circuit conducted very readily, and the action was very energetic. Hydrogen was given off at the platinode in a solution of potassa, and oxygen at the zincode in the sulphate of copper. A small quantity of gas was also seen to rise from the surface of the diaphragm. In about ten minutes the lower surface of the membrane was found beautifully coated with metallic copper, interspersed with oxide of copper of a black color, and hydrated oxide of copper of a light blue.

“The explanation of these phenomena is obvious. In the experimental cell we have two electrolytes separated by a membrane, through both of which the current must pass to complete its circuit. The sulphate of copper is resolved into its compound anion, sulphuric acid + oxygen (oxysulphion), and its simple cathion, copper: the oxygen of the former escapes at the zincode, but the copper on its passage to the platinode is stopped at the surface of the second electrolyte, which for the present we may regard as water improved in its conducting power by potassa. The metal here finds nothing by combining with which it can complete its course, but being forced to stop, yields up its charge to the hydrogen of the second electrolyte, which passes on to the platinode, and is evolved.

“The corresponding oxygen stops also at the diaphragm, giving up its charge to the anion of the sulphate of copper. The copper and oxygen thus meeting at the intermediate point, partly enter into combination, and form the black



oxide; but from the rapidity of the action, there is not time for the whole to combine, and a portion of the copper remains in the metallic state, and a portion of the gaseous oxygen escapes. The precipitation of blue hydrated oxide doubtless arose from the mixing of a small portion of the two solutions."

77. It will be admitted, that agreeably to the admirable researches of Faraday, there are two modes in which a voltaic current may be transmitted, *conduction* and *electrolyzation*. In order that it may pass by the mentioned process, there must be a row of anions and cations forming a series of electrolytic atoms extending from the cathode to the anode. It is not necessary that these atoms should belong to the same fluid. A succession of atoms, whether homogeneous, or of two kinds, will answer, provided either be susceptible of electrolyzation. Both of the liquids resorted to by Daniell, contained atoms susceptible of being electrolyzed. If his idea of the composition of sulphate of copper, and the part performed by the potassa, were admitted for the purpose of illustration, we should, on one side of the membrane, have a row of atoms consisting of oxysulphion and copper; on the other, of oxygen and hydrogen.

78. Recurring to Daniell's own description of the electrolyzing process, above quoted, an atom of copper near the anode being liberated from its anion, oxysulphion, and charged with electricity, seizes the next atom of oxysulphion, displacing and charging an atom of copper therewith united. The cupreous atom thus charged and displaced, seizes a third atom of oxysulphion, subjecting the copper, united with it, to the same treatment as it had itself previously met with. This process being repeated by a succession of similar decompositions and recompositions, an electrified atom of copper is evolved at the membrane, where there is no atom of oxysulphion. Were there no other anion to receive the copper, evidently the electrolyzation would not have taken place; but oxy-

gen on the one side of the membrane, must succeed to the office performed by oxysulphion on the other side; while hydrogen, in like manner, must succeed to the office of the copper.

79. Such being the inevitable conditions of the process, how can it be correctly alleged by Professor Daniell, the transfer of the copper being arrested at the membrane, that as this metal "*can find nothing to combine with,*" it gives up its electrical charge to the hydrogen, which proceeds to the cathode? As hydrogen cannot be present, excepting as an ingredient in water, how can it be said that the copper can discharge itself upon the hydrogen, without combining with the oxygen necessarily liberated at the same time by the electrolytic process? How could the copper, in discharging itself to a cathion, escape a simultaneous seizure by an anion? Would not the oxidizement of this metal be a step indispensable to the propagation of that electrolytic process, by which alone the hydrogen could as alleged, "*pass to the platinode,*" i.e., cathode?

80. In these strictures I am fully justified by the following allegations of Faraday, which I quote from his Researches, 826, 828:

"A single ion, i.e., one not in combination with another, will have no tendency to pass to either of the electrodes, and will be perfectly indifferent to the passing current, unless it be itself a compound of more elementary ions, and so subject to actual decomposition."

"If, therefore, an ion pass towards one of the electrodes, another ion must also be passing simultaneously to the other electrode, although, from secondary action, it may not make its appearance."

81. In explanation of the mixed precipitates produced upon the membrane, I suggest that the hydrated oxide resulted from chemical reaction between the alkali and the acid, the oxide from the oxygen of the water or potassa acting



as a cathion in place of that of the oxide of copper: also that the metallic copper is to be attributed to the solutions acting both as conductors and as electrolytes; so that, at the membrane, two feeble electrodes were formed, which enabled a portion of the copper to be discharged without combining with an anion, and a portion of oxygen to be discharged without uniting with a cathion. In this explanation I am supported by the author's account of a well known experiment by Faraday, in which a solution of magnesia and water was made to act as electrodes at their surfaces respectively.

82. There can, I think, be no better proof that no reliance should be placed on the experiments with membranes, in this and other cases, where the existence of compound radicals in acids is to be tested, than the error into which an investigator, so sagacious as my friend Professor Daniell, has been led, in explaining the complicated results.

83. The association of two electrolytes, and the chemical reaction between the potassa and acid, which is admitted to have evolved the hydrated oxide, seem rather to have created difficulties than to have removed them.

84. In this view of the subject, I am supported by the opinion of Faraday, as expressed in the following language:

“When other metallic solutions are used, containing, for instance, peroxides, as that of copper combined with this or any decomposable acid, still more complicated results will be obtained, which, viewed as the direct results of electro-chemical action, will, in their proportions, present nothing but confusion; but will appear perfectly harmonious and simple, if they be considered as secondary results, and will accord in their proportions with the oxygen and hydrogen evolved from water by the action of a definite quantity of electricity.”

85. I cannot conceive, that in any point of view the complicated and “*confused*” results of the experiment of Daniell with electrolytes separated by membranes, are ren-

dered more intelligible by supposing the existence of salt radicals. I cannot perceive that the idea that the anion in the sulphate is oxysulphion, makes the explanation more satisfactory than if we suppose it to be oxygen. Were a solution of copper subjected to electrolysis alone, if the oxide of copper were the primary object of the current, the result would be analogous to the case of sodium, excepting that the metal evolved at the cathode, not decomposing water, would appear in the metallic form. If water be the primary object of attack, the evolution of copper would be a secondary effect.

86. It is remarkable, that after I had written the preceding interpretation of Daniell's experiments, I met with the following deductions stated by Matteucci, as the result of an arduous series of experiments, without any reference to those of Daniell above mentioned. It will be perceived that these deductions coincided perfectly with mine.

87. I subjoin a literal translation of the language of Matteucci from the *Annales de Chimie et de Physique*, tome 74, 1849, page 110:

"When salt, dissolved in water, is decomposed by the voltaic current, if the action of the current be confined to the salt, for each equivalent of water decomposed in the voltameter, there will be an equivalent of metal at the negative pole, and an equivalent of acid, plus an equivalent of oxygen, at the positive pole. The metal separated at the negative pole will be in the metallic state, or oxidized, according to its nature. If oxidized, an equivalent of hydrogen will be simultaneously disengaged by the chemical decomposition of water."

88. Thus, it seems, that the appearance of acid and oxygen at the anode, and of alkali and hydrogen at the cathode, which has been considered as requiring the simultaneous decomposition of two electrolytes upon the heretofore received theory of salts, has, by Matteucci, been found to be a result



requiring the electrolysis of the metallic base only, and, consequently, to be perfectly reconcilable with that theory.

89. In fact I had, from the study of Faraday's Researches, taken up the impression that the separate appearance of an acid and base, previously forming a salt, at the voltaic electrodes, was to be viewed as a secondary effect of the decomposition of the water or the base; so that acids and bases were never the direct objects of electrolytic transfer.

*Of Liebig's "Principles," so called.*

90. Under the head of the "theory of organic acids," in Liebig's Treatise on Organic Chemistry, we find the following allegations dignified by the name of principles. Manifestly they must tend to convey a false impression to the student, that hydrogen has a peculiar property of creating a capacity for saturation, instead of being only the measure of that capacity, as is actually true, and likewise that in this respect it differs from any other radical.

91. The allegations to which I refer are as follows, being a literal translation from the French copy of the *Traité* of Liebig, page 7:

"The hydrated acids are combinations of one or more elements with hydrogen, in which the latter may be replaced wholly or in part by equivalents of metals.

"The capacity of saturation depends consequently on the quantity of hydrogen which can be replaced.

"The compound formed by the other elements being considered as a radical, it is evident that the composition of this radical can exercise no influence on the capacity of saturation.

"The capacity of saturation of these acids augments or diminishes in the same ratio as the quantity of hydrogen, not entering into the salt radical, augments or diminishes.

"If into the composition of the salt radical there should be introduced an undetermined quantity of any element

without changing the quantity of hydrogen extraneous to the radical, the atomic weight of the acid would be augmented, but the capacity of saturation would remain the same."

92. As by the advocates of the existence of "*salt radicals*," hydrogen is considered as playing the part of a metallic radical, and must, therefore, as respects any relation between it and the capacity of saturation, be in the same predicament as any other electro-positive radical, I cannot conceive wherefore laws, which affect every other body of this kind, should be stated as if particularly associated with hydrogen.

93. Would not a more comprehensive and correct idea be presented by the following language?

94. From any combination of an acid with a base, either the base or its radical may be replaced by any other radical or base, between which and the other elements present, there is a higher affinity. Of course from acids called hydrated, from their holding an atom of basic water, either this base, or its radical (hydrogen), may be replaced by any other competent base or radical.

95. The premises being manifestly fallacious, still more so is the subsequent allegation, that in consequence of the hydrated acids being compounds formed with hydrogen, their capacity of saturation *depends* on the quantity of this element which can be replaced.

96. Is not this an inversion of the obvious truth, that the quantity of hydrogen present is as the capacity of saturation; and that, of course, the quantity of any element which can be substituted for it, must be in equivalent proportion? Would not a student, from this, take up two erroneous ideas—first, that the capacity of saturation is conferred by the radical, and in the next place, that of all radicals, hydrogen alone can give such a capacity? Is it not plain, that the assertion here made by the celebrated author, would be true of any radical?

97. Passing over a sentence which has no bearing on the



topic under discussion, in the fourth allegation we have a reiteration and expansion of the error of those by which it is preceded. We are informed that the "*capacity of saturation augments and diminishes with the quantity of hydrogen which can be replaced,*" which is again an inversion of the truth, that the quantity of hydrogen varying with the capacity, the quantity of any other radical, competent to replace it, must be in equivalent proportion.

98. Is not the concluding allegation a mere truism, by which we are informed, "that if any undetermined quantity of any element should be introduced into the composition of the radical, without changing the capacity (as measured by hydrogen), the capacity would be found the same when measured by any other radical"?

99. As all that is thus ascribed to hydrogen must be equally true of any other radical, there would have been less liability to misapprehension, had the generic term radical been employed wherever hydrogen is mentioned. But by employing the word radical to designate halogen elements, the advocates of the existence of compound radicals in amphoteric salts have deprived the word in question of most of its discriminating efficacy. In fact, their nomenclature would confound all ultimate elements under one generic appellation, and all their binary combinations under another, so that almost every chemical reagent, whether simple or compound, would be a salt or a radical.

100. Before concluding, I feel it to be due to the celebrated German Chemist above mentioned, to add, that however I may differ from him as to the acids being hydrurets of compound radicals, I am fully disposed to make acknowledgments for the light thrown by his analytical researches on organic chemistry, and the successful effect of his ingenious theoretic speculations, in rendering the science more an object of study with physicians and agriculturists."

And Wolcott Gibbs (1843) wrote:

“Dr. Hare has brought forward a number of powerful arguments against the doctrine of compound salt-radicals, which has recently made great progress among European chemists, and at present threatens to subvert all established theories and nomenclature, and to erect the superstructure of chemical science upon a foundation apparently far too unsubstantial to support its gigantic proportions and rapidly increasing weight. This theory sets out from a principle very different from any which chemists have been hitherto accustomed to admit, and which would seem to be involved in a philosophical idea of the province and objects of chemistry, while it aims at explaining a few superficial resemblances in purely *physical* properties, by making a total change in the *chemical* constitution of those substances between which such resemblances exist, as well as of innumerable others which display in their physical relations far more striking discordances. Thus the physical similarity between the chlorides, iodides, &c., of the alkaline and earthy metals, and the sulphates, nitrates and other oxysalts of the same metallic radicals, is made the basis of a total change in our views of the chemical constitution of all salts whatever, while the much more remarkable and more widely extended differences between other members of the same classes of compounds, so forcibly urged and so clearly illustrated by Dr. Hare, are left entirely unnoticed or swallowed up in the sweeping assertion that the salts of the simple haloid type, and the salts composed of amphacids and amphibases, form a series of basic and acid compounds for the most part completely parallel.

The principal arguments which have been brought forward in favor of the salt-radical theory, which is in part based upon this assumed parallelism, have been ably discussed by Dr. Hare in the preceding memoir.”



In what was probably his final letter to Berzelius, Hare said:

“ Philadelphia, May, 1845.

“ *Esteemed Sir*,—I am extremely grateful to you for the good will which has induced you to occupy so much of your valuable time and attention in answering my letters, and regret that I have not succeeded in so expressing myself, whether in French or English, as to make you comprehend my opinions. I shall begin to think that in theoretic elucidation, as well as in physical illumination, it may be more difficult to make luminous impressions on bodies, in proportion as they are themselves pre-eminently the sources of light.

In your letter of the 25th of February, 1844, you describe my opinion of a salt in the following words, “ Vous fondez l’idée d’un sel uniquement sur la composition sans égard aux propriétés, vous ne considérez, comme un sel que ce qui est composé d’une combinaison binaire appelée *base* et une autre combinaison binaire appelée *acide*. Les sels dit haloïde, ne sont pas, d’après vous, des sels, puis qu’ils ne sont composés que de deux éléments et ne contiennent ni base ni acide.” That this account of my opinion is erroneous must appear from the following language, held in my letter to Professor Silliman, which first gave rise to our correspondence on the subject of nomenclature (p. 221).

On reperusing the passages which I have thus annexed, you will perceive, that I have treated as absurd the idea of restricting our conception of a salt to a compound formed of an amphide acid and an amphide base, and that I have denounced that of depriving the chloride of sodium of its appropriate name, and eliminating from the class of salts compounds analogous to this chloride in composition and properties.

In the following paragraphs, taken from my “ *Effort to refute the arguments advanced in favor of the existence, in*

*amphide salts, of a compound radical like cyanogen," I have objected to the employment of the word salt as a cornerstone of any scientific superstructure. "27. It much surprises me, that when so much stress is laid upon the idea of a salt, the impossibility of defining the meaning of the word escapes attention. How is a salt to be distinguished from any other binary compound? When the discordant group of substances which have been enumerated under this name, is contemplated, is it not evident that no definition of them can be founded on community of properties? and, by the advocates of the new doctrine, composition has been made the object of definition, instead of being the basis. Thus agreeably to them, a compound is not a salt, because it is made of certain elements; but, on the contrary, an element, whether simple or compound, belongs to the class of salt radicals, because it produces a salt. Since sulphur, with four atoms of oxygen,  $SO_4$ , produces a salt with a metal, it must be deemed a salt radical.*

*"Evidently the word salt has been so used, or rather so abused, that it is impossible to define it, either by a resort to properties or composition; and I conceive, therefore, that to make it a ground of abandoning terms which are susceptible of definition, and which have long been tacitly used by chemists in general, in obedience to such definition, would be a retrograde movement in science."*

On perusing the preceding passages, you must perceive that the difference between us, is not, that while you would build upon one idea of a salt, I would build upon another; it lies, on my part, in the rejection, as a basis of nomenclature or classification, of a word, so vaguely used, and so undefinable as that in question.

As respects another misapprehension, it never occurred to me, that binary haloid compounds were less entitled to be considered as salts, on account of their having no more than



two elements. The tendency of my opinions has been to consider the chloride of sodium, as the basis of the saline genus and to object to the treatment of any body as a salt, which has not some analogy with it in properties, if not in composition.

The feature in your nomenclature and classification which is most discordant with that which I have proposed, is the distinction which you have attempted to make between the binary compounds formed by halogen bodies with electro-positive radicals and those formed with the same radicals by amphigen bodies. I cannot conceive upon what ground the former, for the most part, are more worthy of being considered as salts than the latter; nor whereupon the amphide compounds resulting in the one case, are to be considered as acids or bases, according to their relation to the voltaic poles, more than are the haloid compounds resulting in the other.

Your nomenclature and your classification are founded on the words *acid*, *salt*, and *base*, and yet you have not given any consistent definition of the ideas to be attached to either. These words have been shown to be employed by you in different senses, whether as respects composition or properties.

On this subject you will find the following comments in my letter to Professor Silliman above quoted:

*"An attempt to reconcile the definition of acidity given by Prof. Berzelius, with the sense in which he uses the word acid, will, in my apprehension, increase the perplexity. It is alleged in his Traite, page 1, Vol. II, 'that the name of acid is given to silica and other feeble acids, because they are susceptible of combining with the oxides of electro-positive metals, that is to say with salifiable bases, and thus to produce salts, which is precisely the principal character of acids.'* Again, Vol. I, page 308, speaking of the *halogene* elements, he declares that 'their combinations with hydrogen, are not only acids, but belong to a series the most puissant that we can employ in chemistry; and in this respect they rank as

equals with the strongest of the acids, into which oxygen enters as a constituent principle.' And again, Vol. II, page 162, when treating of hydracids formed with the halogene class, he alleges, '*The former are very powerful acids, truly acids, and perfectly like the oxacids; but they do not combine with salifiable bases; on the contrary, they decompose them and produce haloid salts.*'

"In this paragraph, the acids in question are represented as pre-eminently endowed with the attributes of acidity, while at the same time they are alleged to be destitute of his '*principal character of acids*,' the property of combining with salifiable bases.

"On page 41 of the same volume, treating of the acid consisting of two volumes of oxygen and one of nitrogen, considered by chemists generally as a distinct acid, Berzelius uses the following language: 'If I have not coincided in their view, it is because, judging by what we know at present, the acid in question cannot combine with any base, either directly or indirectly; that consequently it does not give salts, and that salifiable bases decompose it always into nitrous acid and nitric oxide gas. It is not then a distinct acid, and as such ought not to be admitted into the nomenclature.'"

I suggested a definition here subjoined, which is founded upon your one electro-chemical classification, and which is no more than an enunciation of a rule acted upon, and consequently sanctioned tacitly by yourself, and all other chemists. The definition in its amended form, as given in my text-book, is as follows:

"*When of two substances capable of combining together to form a tertium quid, and having an ingredient common to both, one prefers the positive, the other the negative pole of the voltaic series, we must deem the former an acid, the latter a base; also, any body capable of saturating an acid, as above defined, is a base, and any body capable of saturating a base as above defined, is an acid*" (p. 243).



It follows that agreeably to the nomenclature proposed by Faraday, every acid is an "anion," every base a "cation."

But to proceed to another part of the letter, which I have had the honor to receive from you, it is there alleged that although "*nitrate calcique*" (nitrate of lime) is a deliquescent salt, while fluor spar is a stone, you class them together because they have, in common, the property of yielding with sulphuric acid *gypsum* and a *free acid*. But allow me respectfully to inquire how, consistently with your system, sulphuric acid can extricate a free acid from fluoride of calcium? By your own premises fluoride of calcium is a salt, then wherefore is not the fluoride of hydrogen a salt? If it be a salt, where is the analogy between the reaction of the sulphuric acid with the nitrate of lime and the fluor? In the former case sulphuric acid liberates an acid by a superior affinity for a base already existing; in the latter case, by causing the oxygen of its combined water to unite with calcium, it generates a base and afterwards combines with it; and, while decomposing one fluoride, gives rise to another. In the instance of the nitrate, one amphide salt is replaced by another amphide salt, while an acid is liberated; in the instance of the fluoride, an haloid salt is replaced, both by an amphide salt and another haloid compound. As, according to your system, this compound consists of a halogen, or salt-generating body, combined with a radical, it should be treated as a simple salt.

If, as you stated in your *Traite*, an ability to combine with bases be an essential attribute of acidity, how can the fluoride of hydrogen be an acid, unless my view of the question be admitted, agreeably to which the electro-negative fluorides are fluacids, the electro-positive fluorides, fluobases, while the compound of a fluacid and fluobase is a salt, at least as much as feldspar, or marble. With what other base than a fluobase, can the fluoride of hydrogen unite as an acid, so as to fulfil the conditions of your definition?

I am prevented from supposing that by adopting the salt radical theory, you would rest the analogy of the cases cited, on the existence of a compound radical oxynitron, in the nitrates, because in your letter of the 15th of September, you allege, that you prefer to consider oxysalts as consisting of two oxides. Besides, I hope you will consider the arguments which I have advanced against that theory, as unanswerable.

But admitting the existence of oxynitron in the nitrates, wherefore should not fluorine in fluacids, play the same part as oxygen in oxacids. If a compound radical be formed when two oxides come together, wherefore should there not be a compound radical formed by the meeting of two fluorides? If in the one case, all the oxygen goes to form a compound radical, in the other ought not all the fluorine to perform an analogous part? Hence if on the one hand we admit the existence of oxynitron, on the other we must admit that of fluohydrogenion.

It will be conceded that there is a great analogy between the acid haloid compounds of hydrogen erroneously named hydracids, and those formed by the same radical with sulphur, selenium, and tellurium. I have designated the three last, and likewise water, when acting as an acid, as amphydric acids, while I have designated the haloid hydracids so called, as halohydric acids; founding these appellations on your words amphigen and halogene. Can it be imagined that although when either of the amphydric acids, sulphydric acid for instance, is presented to a corresponding amphide compound, sulphide of potassium for instance, that a compound radical is generated, so that the formula of the resulting sulpho-salt is to be  $HS_2P$ , and yet that when fluohydric acid is presented to the fluoride of potassium, there being no generation of a radical, the formula of the resulting compound is to be  $FH + FP$  —.

You consider it as an objection that I must class the oxide



of sodium with the chloride and sulphide of the same metal, notwithstanding the diversity of their properties; but how can this be a consistent objection, when, according to your nomenclature, the chloride of sodium is classed not only with the fuming liquor of Libavius, the butyraceous and volatile chlorides, which though analogous in composition differ from it in properties extremely, but also with feldspar, gypsum, glass, and marble, which are utterly different from it in composition, as well as in properties?

If in the case of the nitrate of lime and fluorspar we are to overlook that the latter is a stone, the former a deliquescent salt, in consideration of the alleged community of results obtained by reaction with sulphuric acid, let us subject the sulphide and chloride of sodium to the same test. Do we not obtain from either, sulphate of soda and a free acid? Is there not a much greater analogy between chlorohydric acid and sulphydric acid, than between the nitric acid and the fluoride of hydrogen? Under this aspect can it be reasonable to class together the nitrate of lime and fluor spar as simple salts, and yet exclude the sulphides of the same class? Are not the sulphides more analogous to the chlorides and fluorides than the nitrates, in the very *Traite* to which you have referred? I allude to the evolution from either by reaction with sulphuric acid of a like base and of one of the acids improperly called hydracids.

It is considered as objectionable that chloride of sodium, a neutral salt "*par excellence*," should be deemed a base. But I would ask, whence originated the nominal *neutrality* of this chloride; did it not spring from the old abandoned notion of its consisting of muriatic acid and oxide of sodium? That it is a salt *par excellence*, I admit, but deny that it is a neutral salt agreeably to the idea associated with the term neutral as applied to the sulphates of potash and the sulphate of soda, in contradistinction to the acid bisulphates of

these bases. That it is neutral or inert, as respects its re-agence with vegetable colors, ought not, as I conceive, any more to be an objection to its claims to the basic character, than the like inertness is an objection to the basic pretensions of the oxides of the metals proper, among which very few, if any, have any alkaline reaction. This is more properly a test of *alkalinity* than of basidity.

Since water, alumina, and some other oxides, are considered as capable severally of acting as an acid in some compounds and as a base in others, wherefore may not the same substance have the attributes of a salt in one case, and yet in others act as a base? Which is the most remote from the character of a base, is it the salt or the acid?

I am obliged to you for the information given at the close of your letter. I do not know whether you have ever met with the account given in the Bulletin of the proceedings of the American Philosophical Society, of my success in fusing pure rhodium and iridium, by the hydro-oxygen blowpipe.

I have been for sometime endeavoring to perfect some new methods of analyzing organic substances by burning them in oxygen gas.

With highest esteem, I am yours sincerely,

ROBERT HARE."

"On this subject the following remarks were made in my letter on your nomenclature above referred to: 'In common with eminent chemists Prof. Berzelius has distinguished acids in which oxygen is the electro-negative principle as *oxacids*, and those in which hydrogen is a prominent ingredient as *hydracids*. If we look for the word radical, in the table of contents in his invaluable treatise, we are referred to page 218, volume first, where we find the following definition, "*the combustible body contained in an acid, or in a salifiable base, is called the radical of the acid or of the base.*"'

"In the second volume, page 163, hydracids are defined to



be 'those acids, which contain an electro-negative body, combined with hydrogen'; and on the next page it is stated, that 'hydracids are divided into those which have a simple radical, and those which have a compound radical. The second only comprises those formed with cyanogen and sulphocyanogen.' Again in the next paragraph, 'no radical is known that gives more than one acid with hydrogen, although sulphur and iodine are capable of combining with it in many proportions. If at any future day more numerous degrees of acidification with hydrogen, should be discovered, their denomination might be founded on the same principles as those of oxacids.' Consistently with these quotations, all the electro-negative elements forming acids with hydrogen, are radicals and of course by the definition of Prof. Berzelius, combustibles; while hydrogen is made to rank with oxygen as an acidifying principle, and is consequently neither a radical nor a combustible. Yet page 189, volume second, in explaining the reaction of fluoboric acid with water, in which case fluorine unites with hydrogen and boron, it is mentioned as one instance among others in which fluorine combines with two combustibles.

"I am of opinion that the employment of the word hydracid as co-ordinate with oxacid, must tend to convey that erroneous idea, with which, in opposition to his own definition, the author seems to have been imbued, that hydrogen in the one case plays the same part as oxygen in the other. But in reality the former is eminently a combustible, and of course is the radical by his own definition."

Perhaps at this point we may with advantage retrace our steps to observe other activities of Hare's life. For instance, his visit to England (1836) is not recorded. But from the following letters we may gather some idea in regard to it. He must have been happy when in Dalton's company, and it can be imagined that their conversation never lacked for

real earnest and important topics. It is also beautiful to note that, at this time, as at all other times, he made it a point to advise his friend Silliman of his experiences.

He informed Silliman that he mentioned, in his address before the British Association, the mode he pursued in fusing platinum; his recommendation of the use of a nitrite in preparing nitrous ether; his observation of "a species of ether different from the usual ether" arising on exposing a nitrite to the action of alcohol and diluted sulphuric acid; the deposition of carbon when olefiant gas is inflamed with insufficient oxygen; the formation of peculiar products on inflaming the aqueous elements in the presence of an essential oil, all of which pointed to sources of error in gas analysis experiments; and sent him the following letter:

"Philadelphia, August 30th, 1837.

"My dear Friend:

On the 16th of this month, I sent to the venerable and celebrated Dalton, as chairman of the chemical section of the British Association, a letter of which I now send you an extract. My motive for publishing this extract in your Journal, is my impression that I owe it to you and others of my scientific countrymen to communicate the facts which I have stated to men of science in the mother country, and that I owe to the latter a more public acknowledgment than I have yet made, of the grateful recollection which I entertain of the kindness with which I was received at their meeting at Bristol. This I am convinced, was intended as a mark of regard, not merely to me as an individual, but to American cultivators of science in general, of whom I was considered as a representative.

The Marquis of Northampton, who presided, stated to me that if there were others of my scientific countrymen present, he wished to be made acquainted with them, as he felt that it would be his duty to pay them attention.



As respects myself, I was received more like an old acquaintance than as a stranger. I was invited to a seat next the Vice President at the dinner, where I believe about four hundred of the members were present, and requested to sit as a member of the committee of the chemical section. On every occasion, I was treated with great deference and kindness.

In the extract sent you, I have omitted some parts of my letter to Dr. Dalton, as they referred to facts already published in the number of the Franklin Journal for July.

I am faithfully yours,

To Prof. Silliman."

ROBERT HARE."

To John Dalton, Esq., Chairman of the Section on Chemistry of the British Association for the Advancement of Science:

"Dear Sir—

"Philadelphia, August 14, 1837.

I beg leave through you to communicate to the British Association for the Advancement of Science, that by an improvement in the method of constructing and supplying the hydro-oxygen blowpipe, originally invented by me in the year 1801, I have succeeded in fusing into a malleable mass more than three fourths of a pound of platina. In all, I fused more than two pounds fourteen ounces into four masses, averaging of course nearly the weight above mentioned. I see no difficulty in succeeding with much larger weights. The benefit resulting from this process is in the facility which it affords of using platina scraps or old platina ware into lumps, from which it may be remodeled into new apparatus.<sup>8</sup>

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<sup>8</sup> I have, since this statement was made, been led to believe that fused platina will be free from a fault to which Wollaston's platina is more or less liable, accordingly as the process is more or less skilfully managed. The fault to which I allude is that of scaling when extended under the hammer in order to form a crucible or capsule. I

The largest lumps were fused agreeably to my original plan of keeping the gases in different receptacles and allowing them to meet during efflux. I have, however, operated in the large way upon the plan contrived and employed by Newman, Brooke, Clarke and others, having used at one operation nearly thirty gallons of the mixture of the gaseous elements of water.

This I was enabled to do with safety by an improvement in Hemming's safety tube. With this improved plan, I have allowed the gas to explode, as far into the tube of efflux as the point where the contrivance in question was interposed, at least a hundred times without its extending beyond it. Still, however, the other mode in which the gases are separate until they meet in passing out of their respective receptacles, is less pregnant with anxiety, if not with risk. As these elements are known to explode by the presence of several metals, other mysterious causes of explosion may be discovered.

How much do I regret, that an ocean now rolls between myself and those respected and esteemed brethren in science whom this time last year I had the pleasure to meet and greet at Bristol, and to whom I shall ever be grateful for their kind reception. How much would it gratify me, could I exhibit to them and their enlightened visitors, that splendid concentration of light and heat which I have latterly employed, by which a metal infusible in the air furnace or forge, is made as fluid as mercury, so as to be blown off in globules.

With the highest esteem, I am respectfully yours,

ROBERT HARE."

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had a platina dish of nine ounces in which many scales existed. By fusion, this tendency in the metal appeared to be corrected.

During the fusion of some large lumps which had been imperfectly welded from the state of sponge, vitreous globules were observed to exude. Of this fact I can conceive of no other explanation than one founded on the allegation of Prof. Daniell, that during exposure to fire, platina absorbs silicon.



While experimental inorganic chemistry, in its broadest sense, engaged Hare's consideration, yet at times he ventured into the organic domain. Instances of this are cited at various places in the present narrative. On one occasion (1837) he mixed two ounces of oil of turpentine, four ounces of alcohol and eight ounces of sulphuric acid and subjected the mixture to distillation. The distillate was a yellow colored liquid. The admixed sulphurous acid was removed by ammonia, and the ether by heat, when there remained a liquid differing in smell and taste from the oil of turpentine. It had no action on metallic potassium. Examination showed the presence of a small quantity of sulphuric acid in it. Other essential oils behaved similarly altho' there were some with which the results were wholly different. Cinnamon oil from cassia, treated as above, gave no definite product. This was also true of sassafras and cloves. In one instance he got from sassafras "a minute quantity of a lighter liquid, devoid of acid, which burned without smoke, was insoluble in water, and very fluid." He termed it *sassafreine*, analogous to hydric ether. "One drop of oil of sassafras imparted a striking color to 48 ounce measures of sulphuric acid and appeared perceptible when it formed less than a five millionth part." When any of the essential oils were brought in contact with sulphurous acid "they acquired a yellow color." Essential oils containing oxygen were most affected by the action of sulphurous acid.

In this connection Hare said:

"By distilling camphor with alcohol, and sulphuric acid, I obtained a yellow liquid, which, by washing with ammonia and evaporation, in order to get rid of the sulphurous ether, yielded an oil. The oil, by standing, separated into two portions, one solid, the other liquid. The solid portion resembled camphor somewhat in smell, but differed from it by melting at a much lower temperature, becoming completely fluid at 175.

I found that the essential oils of cinnamon and cloves possessed an antiseptic power, quite equal to that of creosote, and that their aqueous solutions, when sulphated, were ever superior to similar solutions of that agent.

One part of milk mingled with four parts of a saturated aqueous solution of the sulphated oil of cloves, remained after five days sweet and liquid, while another portion of the same milk became curdled and sour within twenty four hours. Having on the second of July added two drops of oil of cinnamon to an ounce measure of fresh milk, it remained liquid on the eleventh; and, though it finally coagulated, it continued free from bad taste or smell until September, although other portions of the same milk had become putrid. A half ounce of milk to which a drop of sulphurous oil of turpentine had been added, remained free from coagulation at the end of two days, while another portion, containing five drops of pure oil of turpentine, became curdled and sour on the next day.

A number of pieces of meat were exposed in small wine glasses, with water impregnated with solutions of the various essential oils. Their antiseptic power seemed to be in the ratio of their acidity. The milder oils seemed to have comparatively little antiseptic power, unless associated with the sulphurous acid, which has long been known as an antiseptic.

In cutaneous diseases, and, perhaps, in the case of some ulcers, the employment of the sulphurous sulphated oils may be advantageous.

A respectable physician was of opinion that the sulphurous sulphate of turpentine had a beneficial influence in the case of obstinate tetter.

Possibly the presence of sulphurous acid may increase the power of oil of turpentine as an anthelmintic. Pieces of corned meat hung up, after being bathed with an alcoholic solution of the sulphurous sulphated oil of turpentine, or with solutions of the sulphated oils of cloves or cinnamon, remained



free from putridity at the end of several months. That imbued with cinnamon had a slight odor and taste of the oil.

I am led, therefore, to the impression that the antiseptic power is not peculiar to creosote, but belongs to other acrid oils and principles, and especially to the oils of cinnamon and cloves.

The union of sulphuric acid with these oils appears to render them more soluble in water; whether any important change is effected in their medical qualities by the presence of the acid may be a question worthy of attention.

I have stated my reasons for considering the ammoniacal liquid, resulting from the ablution of the ethereal sulphurous sulphate of etherine with ammonia, as partially composed of hyposulphuric acid. By adding to this ammoniacal liquid a quantity of sulphuric acid, sufficient to produce a strong odor of sulphurous acid, and then a portion of any of the essential oils; a combination ensued, as already described, between the oils and the sulphurous acid liberated by the sulphuric acid, so as to render them yellow and suffocating. The habitudes of cinnamon oil from cassia under these circumstances were peculiar. A quantity of it was dissolved, communicating to the liquid a reddish hue. The solution being evaporated, a gummy translucent reddish mass was obtained, which, by solution in alcohol, precipitated a quantity of salt, and being boiled nearly to dryness, re-dissolved in water, and again evaporated, was resolved into a mass having the friability, consistency and translucency of common rosin; but with a higher and more lively reddish color. Its odor recalls, but faintly, that of cinnamon; its taste is bitter and disagreeable, yet recalling that of the oil from which it is derived. Its aqueous solution does not redden litmus; nor, when acidulated with nitric acid, does it yield, a precipitate with nitrate of barytes.

Of this substance ten grains were exposed to the process

above mentioned, for the detection of sulphuric acid, and were found to yield a precipitate of 6.5 grains of sulphate of barytes.

It may be worth while to mention, that in boiling the sulphated oils with nitric acid, compounds are formed finally, which resist the further action of the acid, and are only to be decomposed by the assistance of a nitrate and deflagration. I conjecture that these compounds will be found to merit classification as ethers formed by an oxacid of nitrogen.

One of my pupils, in examining one of the compounds thus generated, was, as he conceived, seriously affected by it, suffering next day as from an overdose of opium. He also conceived that a cat, to which a small quantity was given, was affected in like manner.

I had prepared an apparatus with the view of analyzing accurately the various compounds above described or alluded to, by burning them in oxygen gas; when, by an enduring illness of my assistant, and subsequently my own indisposition, I was prevented from executing my intentions."

Lengthy comment on sulphurous ether, and sulphate of etherine (the true sulphurous ether) was also made, in which he concluded "that the yellow liquid obtained by distilling equal measures of sulphuric acid and alcohol, consists of oil of wine held in solution by sulphurous ether, composed of nearly equal volumes or weights of its ingredients; also, that the affinity between the ether and the acid is analogous to that which exists between alcohol and water.

The apparent detection of sulphuric acid in the ammonia, justifies a surmise, that the etherine distils in the state of a hyposulphate, which subsequently undergoes a decomposition into sulphurous acid and the sulphate of etherine.

The liquid above alluded to, as resulting from the saturation of the ethereal sulphurous sulphate of etherine by ammonia, and distillation by means of a water bath gradually



raised to a boiling heat, is a very fragrant variety of oil of wine. It differs from that described by Berzelius as the heavy oil of wine of Hennel and Serullas, in being lighter and containing less sulphuric acid. I have a specimen exactly of the specific gravity of water, and have had one so light as to float on that liquid. The oil of wine obtained by ammonia approximates, in its qualities, to the variety which Thénard describes as light oil of wine.

The presence of sulphuric acid in a definite or invariable ratio does not appear requisite to the distinctive flavour or odour of oil of wine."

The following account of his process for manufacturing sweet spirits of nitre will be read with interest:

"The reaction of nitric acid with alcohol is so difficult to regulate, in the ordinary mode of making ether in which the whole of the materials are mingled at the outset of the process, that I was induced about seventeen years ago (1820), to introduce an apparatus in which they were gradually added together within a glass bottle, by means of glass funnels with glass cocks.

Subsequently I adopted a bottle provided with three tubulures, letting the one tubulure communicate, by means of a recurved tube with another tube passing perpendicularly through an open-necked inverted receiver, and entering a bottle surrounded with ice and salt, occupying a suitable vessel. The cavity of the receiver should likewise be occupied by a freezing mixture.

Into each of the remaining tubulures let a glass tube be introduced, ground or luted to fit air tight, and tapering so as to terminate in a capillary orifice near the bottom of the bottle.

Through one of the tubes introduce as much alcohol as will cover the bottom of the bottle, and then, by means of the other tube, introduce as much strong nitric acid as will

cause an effervescence. Should the effervescence threaten to become explosive, the reaction may be checked by the further addition of alcohol, and when the reaction appears to decline too much, it may be re-excited by an additional quantity of acid. By these means, without applying heat, a quantity of nitric ether will soon be condensed in the refrigerated bottle. To convert this ether into a liquid, fully equal to the official sweet spirits of nitre, let it be mingled with seven parts of alcohol, and four of water. The colder the freezing mixture, the greater will be the product; yet more or less may be obtained by refrigeration with cold water.

It may be proper to mention, that at the bottom of the phial an aqueous acid liquor is deposited, upon which the ether swims, and from which it should be carefully separated."

Calling attention to the fact that on heating sodium or potassium nitrate "the first portions of gas (oxygen) extricated are nearly pure" and that the cold white mass—the residue—on solution in water deposits crystals of nitrate after the liquid cools, but that the mother liquor "evaporated to a certain point begins to yield crystals of hyponitrite" (nitrite), Hare emphasizes that the "superior solubility" of either nitrite renders "it practicable to separate them from the nitrate having the same base." He could not comprehend why just about the third of the nitrate is changed by heat to nitrite and suggested that "it would seem as if there were a reaction between the nitrate and hyponitrite (nitrite), which, having co-operated to expel a portion of the contained oxygen, afterwards restrains the evolution of a further portion until the heat is raised to a point capable of effecting such a decomposition as to evolve the nitrogen and oxygen in a state of mixture."

He came to prefer the use of a real nitrite instead of a nitrate, with sulphuric acid and alcohol, in forming what is "commonly known as nitrous or nitric ether." He obtained



by this means an ether differing from the "ordinary nitrous or nitric ether." "It has a more bland and saccharine taste, milder odour, and greater volatility. . . . Touched with the finger, or tongue, it hisses as does water with a red hot iron."

"When the new ether is distilled from powdered quick lime, this earth imbibes an essential oil, which, with the aid of water, is yielded to pure hydric ether. Of course it is easy to remove this solvent by evaporation or distillation.

The odour of this oil seems to be an ingredient in that of ordinary nitric ether. . . . I suspect that the essential oil in question is one of the impurities which causes the boiling point of the ether generated by nitric acid and alcohol to be higher than the boiling point of that obtained, as in my process, by nascent hypo-nitrous acid (nitrous acid).

When the heat is raised, after the volatile ether ceases to come over from the materials above mentioned as producing it, ethereal products are distilled, of which the boiling point gradually rises as the process proceeds. Meanwhile, the product thus obtained becomes more and more acrid, till at last it is rendered insupportable to the tongue, as respects the after taste. On mingling these liquids with a solution of green sulphate of iron, the ether is all absorbed; but an acrid liquid, which causes the after taste, is not absorbed, and may be separated by hydric ether. The ether being vaporized by heat, the acrid liquid remains. The smallest drop of this liquid is productive of an effect upon the organs of taste and smell like that of mustard or horse-radish.

The new ether, when secured in a glass phial, by means of a well ground stopper, does not undergo any change by keeping in a cool situation for several months. A phial was suspended about fifteen feet below the surface of the ground, in a cistern of water, for about five months; another was left in a cool cellar for a longer period, without any apparent

change of properties. In this case pressure prevented the escape of the ethereal gas as above mentioned.

When the ingredients for generating the new hyponitrous ether are refrigerated below freezing, and left to react, the ether begins to be formed as soon as the temperature rises, and if the aggregate be included in a bottle with an air-tight stopple, a stratum of ether will soon form and swim upon the surface of the mixture. The quantity which can be thus obtained is much less than that which ensues from the employment of the same quantity of materials with a retort and refrigerated receiver; because the elaboration and condensation require a greater difference of temperature than can be imparted, conveniently, to the different portions of a bottle, especially where the upper is required to be the colder portion.

In order to obtain a quantity of ether in a summary way, I resorted to this process last winter, employing about a gallon of the mixture. After I had decanted the ether which formed in the course of a night, the residue, although surrounded by snow, continued to give out the aerial ether for at least a fortnight. The gaseous ether seems to be formed in innumerable, invisible bubbles throughout the mass, which, on this account, presented the singular phenomenon of an elastic liquid. On inserting the stopple, the liquid in the neck of the bottle would subside in the most striking manner, and on removing the stopple, an opposite movement was observable.

All the ethereal compounds formed by the reaction of the oxacids of nitrogen with alcohol appear to be decomposable by green sulphate of iron. Under these circumstances, according to Berzelius, a malate of iron is formed from common nitric ether.

Concentrated sulphuric acid absorbs the elements derived from the alcohol, and liberates nitric oxide gas, which is, it is well known, rapidly absorbable by the green sulphate above mentioned. Let there be three cylindrical glass jars, Nos. 1,



2, and 3, of such a ratio to each other, in size, as to allow two interstices of about half an inch between the second, or intermediate jar, No. 2, and the outer, No. 1, and innermost jar, No. 3; likewise, let two bell glasses be provided, of such a size as that one of them, (A) may enter the inner interstice, while the other, (B) will cover (A) and descend into the outer interstice. Let a wine-glass containing the ether be placed in jar No. 2, and let No. 1 be supplied with green sulphate of iron, the other two with concentrated sulphuric acid, and let the bells be put in their respective places.

Under these circumstances, the ether will be gradually vaporized, and the alcoholic elements, with some oxygen, will be absorbed by the acid, while nitric oxide, being liberated, will pass into the sulphate, and be consequently absorbed.

From the new ether my young friend, Mr. Boyé, who was, at the time, one of my operative pupils, succeeded in evolving alcohol by digestion with slacked lime, and subsequent distillation. The lime was found to be in the state of a hyp-nitrite, giving a precipitate with the nitrate of silver.

When, into a bell-glass containing some of the aeriform ether, a globule of potassium was introduced, and touched with a red hot knob which formed the termination of an iron rod, ignition took place, and the gas seemed to have changed its character. I had not, however, leisure to examine it eudiometrically. There was an odour produced which reminded me both of that of fish and soap."

He also recounted how he had observed an ethereal liquid subsiding on the addition of pure pyroxylic spirit to an aqueous solution of hypochlorous acid, obtained by passing chlorine into water in contact with mercuric oxide.

Having separated the ether thus produced, it was found to have an agreeable and peculiar fragrance. Like oil of wine, it could not be distilled without decomposition. There was an effervescence at the temperature of 140° F.; but the

boiling point rose beyond that of a boiling water-bath. When a naked flame was applied, the ether, previously colourless, acquired a yellowish wine colour, and, by the crackling evolution of vapour, indicated decomposition.

When the liquid hypochlorous acid was subjected to the process of distillation, before the addition of the spirit, an ether resulted which floated on the solution, and which appeared to differ from that obtained as first mentioned.

These observations, and those previously communicated respecting the hyponitrite of methyl, were made by the aid of a small quantity of pure pyroxylic spirit, supplied to him by his friend, Dr. Ure, who regretted that both ill health and the exhaustion of his stock of spirit had prevented him from making further observations and experiments, tending to decide whether the ethers obtained, as he had described, were either or both hypochlorites, or whether mercury entered into the composition of the heavier ether. This there was some reason for believing; since, when boiled to dryness at a high temperature, a reddish residuum was apparent, which being redissolved, and a small strip of copper immersed in the resulting solution, a minute deposition, apparently metallic, was observable."

In another communication he announced:

"That he had procured by means of hyponitrite of soda, diluted sulphuric acid, and pyroxylic spirit, an ethereal liquid in which methyl ( $C_2H_3$ ) might be inferred to perform the same part as ethyl ( $C_4H_5$ ) in hyponitrous ether.

The compound . . . had a great resemblance to alcoholic hyponitrous ether, similarly evolved, in colour, smell and taste; although there was still a difference sufficient to prevent the one from being mistaken for the other.

Pyroxylic spirit appeared to have a greater disposition than alcohol to combine with the ether generated from it, probably in consequence of its having less affinity for water.



The boiling point appeared to be nearly the same in both of the ethers; and in both, in consequence of the escape of an ethereal gas, an effervescence, resembling that of ebullition, was observed to take place at a lower temperature than that at which the boiling point became stationary. The ethereal gas, mentioned in his communication respecting hyponitrous ether, seemed to have escaped the attention of European chemists; and, even after it had been noticed by him, seemed to be overlooked by Liebig, Kane and others in their subsequent publications."

Hare attached the more importance to his success in producing the ether which was the subject of his communication; since, agreeably to Liebig, no such compound exists, and it is to be inferred that this would excite no surprise, when the difference was considered between the consequences of the reaction of nitric acid with pyroxylic spirit, and with alcohol.

"The liquid last mentioned is now viewed as a hydrated oxide of ethyl, while pyroxylic spirit is viewed as a hydrated oxide of methyl. When alcohol is presented to nitric acid, a reciprocal decomposition ensues. The acid loses two atoms of oxygen, which by taking two atoms of hydrogen from a portion of the alcohol, transforms it into aldehyde; while the hyponitrous acid, resulting inevitably from the partial deoxygenization of the nitric acid, unites with the base of the remaining part of the alcohol. But when pyroxylic spirit is presented to nitric acid, this acid, without decomposition, combines with methyl, the base of this hydrate; so that, as no hyponitrous acid can be evolved, no hyponitrite can be produced. Thus in the case of the one there can be no ethereal hyponitrite, in that of the other, no ethereal nitrate."

Hare regretted that Liebig should not have been informed of the improved process for hyponitrous ether, to which he had referred. . . . Instead of recommending a resort to that process, it was advised that the fumes, re-

sulting from the reaction of nitric acid with fecula (starch), should be passed into alcohol, and the resulting vapour condensed by means of a tube surrounded by a freezing mixture.

This process Hare had repeated, and found the product very inferior in quantity and purity to that resulting from the employment of a hyponitrite. In this process, nascent hyponitrous acid, as liberated from a base, is brought into contact with the hydrated oxide. In the process recommended by Liebig, evidently this contact could not take place; since it was well known "that hyponitrous acid could not be obtained by subjecting fecula and nitric acid to distillation, and condensing the aeriform products."

A test for the detection of minute quantities of opium "not exceeding that contained in ten drops of laudanum in a half gallon of water" was devised by Hare. The process was based on the insolubility of lead meconate. The precipitation, where the quantity is small, may require from six to twelve hours, and may be facilitated by a very gentle stirring with a glass rod. When the meconate has settled at the bottom of the vessel, let about thirty drops of sulphuric acid be poured on it by means of a glass tube. Follow this with as much "red sulphate of iron." The meconic acid liberated by the sulphuric acid will give a "striking red colour with the iron salt." This demonstrated the presence of the acid, "and consequently of opium."

In this connection, it may be said that Hare proposed "an easy method of obtaining meconic acid," which consisted in adding to an aqueous infusion of opium a solution of "subacetate of lead." Copious, lead meconate then separated. This was collected upon a filter and exposed to the action of hydrogen sulphide when meconic acid was set free. Its aqueous solution had a reddish amber colour, and on evaporation yielded crystals of the same hue.

Hare recommended the following course to "denarcotise



laudanum": Treat opium shavings four times successively to as much ether, sp. gr. 0.735, as will cover it, allowing each portion to act upon it for about 24 hours. Afterwards treat the residual opium with as much duly diluted alcohol as will be necessary to convert it into laudanum. From the ether extracts crystals separate—this is "the principle distinguished by Robiquet, since called narcotine." "The first use of the denarcotised laudanum was by way of an enema of thirty drops, in the case of a child tortured by ascarides, to whom it gave easy relief." A friend—a veteran in the art of healing—informed Hare that from his use of the denarcotised laudanum "I am led to anticipate the great desideratum in the use of opium is obtained."

In 1837 Hare made numerous communications which appeared either in the *American Journal of Science* or in the "Proceedings or Transactions of the American Philosophical Society." Most of these relate to highly interesting and very important observations. For example, he said, "in a circuit made through a saturated solution of chloride of calcium, by means of a coarse platina wire (No. 14) and a fine wire (No. 26) that when the latter was made the cathode and the former the anode, a most intense ignition resulted. . . . But when the situations of the wires were reversed, so that the smaller wire was made to form the anode, the ignition became comparatively so feeble as to be incompetent to fuse the fine wire. This phenomenon had continued to appear inexplicable, when during the last winter, it occurred to me that the evolution and combustion of the *calcium* might be the cause of the superior heat produced at the cathode."

This led him to substitute calcium chloride for the lime in the process of Seebeck, Berzelius and Tontin. Operating with a deflagrator of three hundred and fifty Cruikshank pairs, of seven inches by three, he speedily obtained a mercurial amalgam. After its exposure to air till all the calcium had

been separated, and igniting the resulting powder to expel the last traces of mercury "the ratio of the weight of lime thus obtained, to the mercury with which it had been united, was not over a five hundredth part." All this prompted Hare to study Davy's Bakerian lecture with exceeding care. It will be recalled that Davy sought to get calcium, strontium and barium by electrolyzing the oxides of these metals in contact with a mercury cathode. In speaking of calcium Davy said: "In the case in which I was enabled to distill the mercury from it to the greatest extent, the tube unfortunately broke while warm, and at the moment when the air entered the metal, which had the colour of silver, took fire and burnt, with an intense white light, into quicklime." This scheme Hare thought a failure, as he did the work of Davy in attempting to isolate strontium and barium. In commenting on the latter Hare wrote: "Had the barium obtained by Davy been free from mercury, it would not have been fusible below a red heat, as alleged by him. Agreeably to my experience, that metal requires no less than a good red heat for its fusion."

And then he proceeds to tell how he operated. The story is fascinating. In this process Hare uses mercury as cathode in an aqueous salt solution. It is probable that this was the first time that that metal had been so employed, and would it not then have to be regarded as the forerunner of its use in making caustic soda from an aqueous sodium chloride solution? Was it not also the forerunner of the employment of mercury as cathode in electro-analysis? It will be recalled that in 1841 Wolcott Gibbs acted as student assistant in Hare's laboratory, and it does seem quite probable that the things which had so deeply interested Hare and occupied so much of his thought would be the subject of discussion with his assistants, so that in later years when Wolcott Gibbs was enriching the domain of analytical chemistry with his contributions he may have recalled his old Philadelphia experi-



ences and used mercury, placed in a small beaker as cathode, in the electrolysis of copper and nickel sulphates, and later Drowne (his former pupil) used it in the electrolysis of iron phosphate, while thousands of determinations and separations of metals have since been expeditiously and accurately effected in this way. Surely it is not too much to claim for Hare the pioneer work in the use of mercury as cathode in industrial and analytical operations. It is, however, inadvisable to reproduce here his apparatus as it has been recently set forth in detail elsewhere.<sup>9</sup>

Suffice to state that the current came from the alternate action of two deflagrators. The amalgam formed was subsequently distilled from an alembic protected by a stout capsule of iron.<sup>10</sup>

The isolated calcium, strontium and barium rapidly oxidized in water or in any liquid in which they were present. The metals all sank in sulphuric acid. They were quite brittle. For fusion they required at least a red heat. After being kept in naphtha their immersion in water was accompanied at first with much less effervescence. They reacted violently in hydrochloric acid.

At present the properties of calcium are quite well known; but of strontium and barium not much can be said. All are silver grey in color. At 800° C. calcium may be drawn into wire and beaten into almost any shape. It is probable that Hare's method of getting strontium and barium may in time prove quite feasible.

He remarked that "By means of solid carbonic acid, I froze an ounce measure of the amalgam of calcium, hoping to effect a partial mechanical separation of the mercury by straining through leather, as in the case of other amalgams. The result, however, did not justify my hopes, as both metals

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<sup>9</sup> Chemistry in America. D. Appleton & Co.

<sup>10</sup> Chemistry in America. D. Appleton & Co.

were expelled through the pores of the leather simultaneously, the calcium forming, forthwith, a pulverulent oxide, intermingled with and discoloured by mercury in a state of extreme division.

By the same means I froze a mass of the amalgam of ammonium as large as the palm of my hand, so as to be quite hard, tenacious and brittle. The mass floated upon the mercury of my mercurial pneumatic cistern, and gradually liquefied, while its volatile ingredients escaped."

Hare also employed the following processes to get calcium: the deflagration of the phosphuret of calcium in an atmosphere of hydrogen; the exposure of the anhydrous iodide of calcium to a current of hydrogen, or ammonia in an incandescent tube; the ignition of the pure earth or its carbonate or nitrate with sugar or of the tartrate and acetate per se. Hence resulted carburets, which, after washing with acetic acid and rubbing on a porcelain tile, displayed the lustre of plumbago, intermingled with metallic spangles, of a brilliancy rivalling that of the perfect metals. The carburets or the spangles thus obtained, were insoluble in acetic or chlorohydric acid, but yielded to aqua regia. The carburets were excellent conductors of the voltaic fluid, as evolved by a series of 100 pairs; and, by deflagration in a receiver filled with hydrogen, yielded metallic particles, which, rubbed on a porcelain tile, formed spangles of a metallic brilliancy. By igniting antimony with tartrate of lime, Hare procured an alloy of that metal with calcium, and expected by analogous means to alloy the metals of the earths with various metals proper. He believed that no effort to obtain calcium prior to his, had been more successful than the abortive experiment of Sir H. Davy. . . . That the spangles obtained by Hare from lime, were calcium, was ascertained by their solution in aqua regia, and the successive subsequent addition of ammonia and oxalic acid; the resulting precipi-



tate being ignited, then redissolved and again precipitated as at first. No precipitate ensued from the addition of ammonia prior to that of the oxalic acid. Sulphydric acid produced a slight discoloration, but gave no precipitate. That the substances, resulting from the ignition of the carbonate with sugar, and washing with acetic acid, contained calcium in the metallic state, combined with carbon, was evident from their being insoluble in acetic or chlorohydric acid; from the deposition of carbon, and giving a precipitate of oxalate of lime on being subjected to aqua regia, ammonia, and oxalic acid; from their metallic brilliancy when burnished, and from their being excellent conductors of the voltaic fluid. By the ignition of the carbonates of baryta and strontia severally with sugar, Hare had attained analogous results to those above mentioned in the case of the similar ignition of carbonate of calcium.

The extreme avidity of calcium for iron was quite striking; since, when a crucible was inclosed in a clean iron case without a cover, the mass, swelling up so as to reach the iron, became slightly imbued with it. By intensely igniting the carburet of calcium, obtained from the carbonate and sugar, with an equal weight of dry tannogallate of iron, the whole of the aggregate became so magnetic that every particle was transferred from one vessel to another by means of a magnet. The mass was filled with minute metallic globules, which yielded only partially to chlorohydric acid, and which, when dissolved in aqua regia, gave, after adding ammonia and filtration, a precipitate with oxalic acid.

Hare was aware that it did not seem consistent that spangles of calcium, burnished upon porcelain, should retain their lustre; as, under other circumstances, and especially when amalgamated, that metal was found to oxidize as soon as exposed to the air. He had, however, through the kindness of J. C. Booth, a pupil of Wöhler, procured a specimen of mag-

nesium evolved by that celebrated chemist. This specimen yielded, under the burnisher, spangles of a lustre as enduring as that observed by Dr. Hare, in the case of calcium.

Two years later, 1839, another remarkable contribution appeared from the laboratory of Hare. It related to what he designated the "deflagration" of carburets, phosphurets, or cyanides in an atmosphere of hydrogen or *in vacuo*, taking occasion furthermore to discuss again the isolation of calcium. His hope was that chemists would find something worthy of attention in this "new mode of applying the voltaic current." The apparatus he designed may be thus described:

"Upon a hollow cylinder of brass an extra air-pump plate was supported. The cylinder was furnished with three valve cocks, and terminated at the bottom in a stuffing-box, through which a copper rod slid so as to reach above the level of the air-pump plate. The end of the rod supported a small horizontal platform of sheet brass, which received four upright screws. These, by pressure, on brass bars extending from one to the other, were competent to secure upon the platform a parallelopiped of charcoal. Upon the air-pump plate a glass bell was supported, and so fitted to it, by grinding, as to be air-tight. The otherwise open neck of the bell was also closed air-tight by tying about it a caoutchouc bag, of which the lower part had been cut off, while into the neck a stuffing-box had been secured air-tight. Through the last mentioned stuffing-box a second rod passed, terminating within the bell in a kind of forceps, for holding an inverted cone of charcoal.

The upper end of this sliding rod was so recurved as to enter some mercury in a capsule. By these means and the elasticity of the caoutchouc bag, this rod had, to the requisite extent, perfect freedom of motion.

The lower rod descended into a capsule of mercury, being,



in consequence, capable of a vertical motion, without breaking contact with the mercury. It is moved by the aid of a lever connected with it by a stirrup working upon pivots.

Of course the capsules may be made to communicate severally with the poles of one or more deflagrators. The substance to be deflagrated was placed upon the charcoal forming the lower electrode, being afterwards covered by the bell. By means of the valve-cocks and leaden pipes a communication was made with a barometer gage; also with an air-pump, and with a large self-regulating reservoir of hydrogen.

The air being removed by the pump, a portion of hydrogen was admitted, and then withdrawn. This was repeated, and then the bell glass, after as complete exhaustion as the performance of the pump would render practicable, was prepared for the process of deflagration *in vacuo*. But, if preferred, evidently hydrogen or any other gas may be introduced from any convenient source by a due communication through flexible leaden pipes and valve-cocks."

Having described the apparatus, Hare continued: "I will give an account of some experiments, made with its assistance, which, if they could have illuminated science as they did my lecture room, would have immortalized the operator. But, alas, we may be dazzled, and almost blinded by the light produced by the hydro-oxygen blow-pipe, or voltaic ignition, without being enabled to remove the darkness which hides the mysteries of nature from our intellectual vision. . . .

An equivalent of quicklime, made with great care, from pure crystallized spar, was well mingled, by trituration, with an equivalent and a half of bichloride of mercury, and was then enclosed within a covered porcelain crucible. The crucible was included within an iron alembic, such as has been described by me.

The whole was exposed to heat approaching to redness.

In two experiments the residual mass had such a weight as would result from the union of an equivalent of cyanogen with an equivalent of calcium.

A similar mixture being made, and, in like manner, enclosed in the crucible and alembic, it was subjected to a white heat. The apparatus being refrigerated, the residual mass was transferred to a dry glass phial with a ground stopper.

A portion of the compound thus obtained and preserved was placed upon the parallelopiped of charcoal, which was made to form the cathode of two deflagrators of one hundred pairs, each of one hundred square inches of zinc surface, co-operating as one series.

In the next place, the cavity of the bell glass was filled with hydrogen, by the process already described, and the cone of charcoal being so connected with the positive end of the series as to be prepared to perform the office of an anode, was brought into contact with the compound to be deflagrated. These arrangements being accomplished, and the circuit completed by throwing the acid upon the plates, the most intense ignition took place.

The compound proved to be an excellent conductor; and during its deflagration emitted a most beautiful purple light, which was too vivid for more than a transient endurance by an eye unprotected by deep-coloured glasses. After the compound was adjudged to be sufficiently deflagrated, and time had been allowed for refrigeration, on lifting the receiver minute masses were found upon the coal, which had a *metallic appearance*, and which, when moistened, produced an efflu-vium, of which the smell was like that which had been observed to be generated by the silicuret of potassium.

Similar results had been attained by the deflagration, in a like manner, of a compound procured by passing cyanogen over quicklime, enclosed in a porcelain tube, heated to incandescence.



Phosphuret of calcium, when carefully prepared, and, subsequently, well heated, was found to be an excellent conductor of the voltaic current. Hence it was thought expedient to expose it in the circuit of the deflagrator, both in an atmosphere of hydrogen and *in vacuo*. The volatilization of phosphorus was so copious as to coat nearly all the inner surface of the bell-glass with an opaque film.

The phosphuret at first contracted in bulk, and finally was, for the most part, volatilized. On the surface of the charcoal, adjoining the cavity in which the phosphuret had been deflagrated, there was a light pulverulent matter, which, thrown into water, effervesced, and, when rubbed upon a porcelain tile, appeared to contain *metallic spangles*, which were oxidized by the consequent exposure to atmospheric oxygen.

In one of my experiments portions of the carbon forming the anode appeared to have undergone complete fusion, and to have dropped in globules upon the cathode. When rubbed, these globules had the colour and lustre of *plumbago*, and, by *friction on paper*, *left traces resembling those produced by that substance*.

About 1822, Professor Silliman had obtained globules, which were at first considered as fused carbon, but were subsequently found to be depositions of that substance transferred from one electrode to the other. Several of these globules were, by him, sent to me for examination, of which none, agreeably to my recollection, appeared so much like products of fusion as those lately obtained.

Formerly plumbago was considered as a carburet of iron, but latterly, agreeably to the high authority of Berzelius, has been viewed as carbon holding iron in a state of mixture, not in that of chemical combination. It would not, then, be surprising if the globules which I obtained consisted of carbon converted *from the state of charcoal into that of plumbago*."

At present artificial graphite is tracing characters upon paper in the millions of pencils in use. Acheson, in 1896, showed the world how such graphite might be prepared from amorphous carbon; but seventy-seven years before this, Hare, with his deflagrator, converted charcoal into plumbago (graphite) and with it traced characters upon paper. His epoch-making discovery, however, was forgotten! With his unique form of primary battery and all its disadvantages this earnest worker obtained as we have just noted in what is truly an electrical furnace, *calcium carbide*, *phosphorus*, *graphite* and *calcium* metal. Was he not indeed the first American experimenter and discoverer in the great field of electro-chemistry?

On another occasion Hare said: "It did not appear to him that sufficient attention had been paid by artists or men of science, to the great difference which existed between the effect upon glass of heating it by radiation and by conduction. When exposed to radiant heat alone, unaccompanied by flame, or a current of hot air, glass is readily penetrated by it, and is heated, within and without, with commensurate rapidity; but, in the case of its exposure to an incandescent vapour or gas, the caloric could only penetrate by the process of conduction; and consequently, from the inferior conducting power of glass, the temperature of the outer and inner portions of the mass would be so different, as by the consequent inequality of expansion to cause the fracture, which was well known, under such circumstances, to ensue.

The combustion of anthracite coal, in an open grate, in his laboratory, having four flues of about 4.12 by 2.12 inches each, in area, just above the level of the grate (the upper stratum of the fire, having nothing between it and the ceiling) had allowed him to perform some operations with success, which formerly he would have considered impracticable. The fire having attained to that state of incandescence to which



it easily arrives when well managed, he had, on opening a hole by means of an iron rod, so as to have a perpendicular perforation extending to the bottom of the fire, repeatedly fused the beaks of retorts of any capacity, not being more than three gallons, causing them to draw out, by the force of gravity, into a tapering tube; so that, on lifting the beak from the fire, and holding the body of the retort upright, the fused portion would hang down so as to form an angle with the rest of the beak, or to have any desired obliquity. By these means, in a series of retorts, the beak of the first might be made to descend through the tubulure of a second; the beak of the second through that of a third, and so on; the beak of the last retort in the row being made, when requisite, to enter a tube passing through ice and water in an inverted bell glass.

By means of the anthracite fire, thick rods, as well as stout tubes, might be softened and extended, or bent into suitable forms.

The lower end of a green glass phial, such as is used usually for Cologne water, might be made to draw out into a trumpet-shaped extremity. A Florence flask might be heated, and made flat, so as to answer better for some purposes. The drawing out of tubes into a tapering form, suitable for introducing liquids through retort tubulures, was thus easily effected; and in all cases the sealing of large tubes was better commenced in this way, although the blowpipe might be necessary to close a capillary opening which could not be closed by the fire."

To effect the congelation of water by the evaporation of ether, Hare said it had been usual to expose a bulb, containing water and moistened by the ether, to a current of air. Recently he had succeeded far more satisfactorily by exposing a quantity of water, twenty times as large as that usually employed, covered by ether in a capsule to a blast of air, proceeding from a vessel in which it had been condensed by

a pressure equal to one or two atmospheres. By these means, the freezing of the water might be seen by five hundred spectators.

He further said that about two years since, he had published an account of a new process for freezing water by the evaporation of ether, caused by a diminution of atmospheric pressure. In the process then described, concentrated sulphuric acid was interposed between the retort holding the water and ether, and the air pump. Since that time he had rendered the process more rapid and interesting by interposing an iron mercury bottle, with two cocks between the receiver holding the acid and the pump. The ether and water were introduced into the retort. . . . But the result which gave increased interest to the process, was the inconceivable rapidity with which the acid, under these circumstances, absorbed the ethereal vapour, which it appeared to do with greater avidity as the process advanced.

The water, in the act of congeling, flew all over the inner surface of the retort, in consequence of an explosive evolution of ethereal vapour, generated amid the aqueous particles. The congelation of the water was rendered evident to the ears as well to the eyes of his class of more than three hundred students.

It was not an uncommon thing for Hare, at meetings of the American Philosophical Society, to describe experiments carried out by him or by his pupils.

Thus, he showed that the vapour of nascent steam generated by the oxy-hydrogen flame was not productive of electricity. "A single-leaf electrometer, more susceptible than the condensing electrometer, was not indicative of any electrical excitement."

He further demonstrated "that foggy air is not a conductor of electricity." The language of his experiment was:



"A cup of hot water, to supply vapour, was placed within a large bell glass, having an open neck of above three inches in diameter; so that the centre of the neck might be immediately under the positive conductor of a large electrical machine. A knob, communicating with the negative conductor, was supported in the centre of the bell glass. Next a red-hot rod of iron terminating in a knob, was suspended by a wire from the positive conductor, so as to descend, concentrically, through the neck, until within striking distance of the knob, above mentioned.

It will be perceived that, in consequence of the high temperature of the rod, and the heat radiating from it to the neck of the bell glass, no moisture could condense upon either, so as to impair the power of the former to give sparks, or of the latter to act as a non-conductor.

The apparatus being thus prepared, and the machine in operation, sparks were found to pass through the foggy air occupying the cavity of the bell glass, as if no moisture had been present.

From the fact that the aqueous vapour does not impair the insulating power of air, he conceived, must justify some important meteorological inferences."

Nearly every teacher of chemistry has experienced, some time in his life, a desire to place before his students a text-book, in a sense, representative of his mode of presenting his thoughts. Many yield to this natural inclination and thus the field soon becomes congested with texts. To Hare, obliged to offer his subject in lecture form, with a dearth of texts to follow, there must have come the feeling that his students ought to be provided with at least a syllabus of the material he laid before them, so quite early in his career, probably in the year 1822, he had printed "for the use of his pupils" his "Minutes of the Course of Chemical Instruction" in vogue in the University. This booklet, octavo in

form, passed through several revisions and had 24 pages with numerous blank pages for student notes. The writer has been fortunate enough to obtain a copy of this little classic and has found its perusal absorbingly interesting. There appears on the first printed page these words:

INTRODUCTORY LECTURE,  
ON THE RISE, PROGRESS, AND PRESENT STATE,  
OF CHEMISTRY, AS AN ART, AND AS A SCIENCE.

FIRST LECTURE,  
ON THE STUDY OF CHEMISTRY.

SECOND LECTURE,  
ON THE CAUSE OF THE PHENOMENA AND OPERATIONS  
OF THE PHYSICAL WORLD.

The definition of chemistry reads: "It treats of the phenomena, and operations of nature, which arise from reaction between the particles of inorganic matter."

Crystallization and affinity are developed quite interestingly and in the fifth lecture the "Atomic Theory" is accorded full consideration. The laws of Dalton are concisely stated. For example, under multiple proportions, it instances the compounds formed by Carbon = 6 and Oxygen = 8, and those by Nitrogen = 14 and Oxygen = 8, and continues:

"Whichever of the substances we keep fixed as to its number, the other or altering quantity will change so as to give multiples, or exact aliquot parts of the first number; thus,

Ox. 8 and N. 14	—	N. 14—Ox. 8
" " " 7	—	" " 16
" " " $3\frac{1}{2}$	—	" " 24
" " " $1\frac{3}{4}$	—	" " 32
		" " 40

the latter is preferable.

Then follows a table of about 50 known simple bodies with their atomic numbers:



Hydrogen .....	1	Yttrium .....	32	Lead .....	104
Oxygen .....	8	Zirconium .....	28	Manganese ....	28
Chlorine .....	35	Fluorine .....	16	Mercury .....	200
Sulphur .....	16	Antimony .....	44	Molybdiun ....	47
Nitrogen .....	14	Arsenic .....	38	Nickel .....	40
Carbon .....	6	Bismuth .....	71	Osmium .....	..
Iodine .....	125	Cadmium .....	56	Palladium .....	56
Phosphorus ....	12	Potassium .....	40	Platinum .....	96
Sodium .....	24	Cerium .....	46	Rhodium .....	44
Lithium .....	11	Chromium .....	28	Selenium .....	40
Barium .....	70	Cobalt .....	30	Silver .....	110
Strontium .....	44	Columbium ....	144	Tin .....	59
Magnesium ....	12	Copper .....	64	Titanium .....	144
Calcium .....	20	Gold .....	200	Tungsten .....	96
Aluminium ....	18	Iridium .....	30	Uranium .....	125
Glucinum .....	16	Iron .....	28	Zinc .....	33
Silicium .....	8				

These numbers were said to be “expressive of the relative combining quantities of the bodies.”

Chemists interested in atomic numbers, to-day, will surely find much to rivet attention in the preceding table and also much that will afford food for quiet, earnest reflection. There is, further, a presentation of physical phenomena in considerable detail.

Each successive edition of the “Minutes” was greatly enlarged, but to advantageously elucidate this text Hare issued, in 1826, a work in two volumes, each of 52 octavo pages, comprising engravings and descriptions of apparatus and experiments. Its exact title was: “Engravings and Descriptions of a great part of the Apparatus used in the Chemical Course of the University of Pennsylvania. With appropriate theoretical explanations.” Upon examining this attractive work it is at once seen that Part I is devoted to physical apparatus and experiments, while Part II considers chemical experiments and apparatus. Care is preserved to mark those portions of apparatus contrived by the author,

or modified by the author and his friend, Silliman. At this late day the volume will interest all who care to examine it. It is proof of the fondness of Hare for striking and elaborate experimentation. Its illustrations are very suggestive.

The success which followed the use of the "Minutes" together with the two volumes of "Engravings," induced Hare to publish his expanded lectures under the title of "Compendium of Chemistry," which first appeared in 1827. It was primarily intended for classes in medicine, numbering from three to four hundred. Those were not the days when laboratory exercises prevailed and were pursued by those who studied the Science, hence the necessity of fully illustrating the lecture-room teaching and the necessity of carefully imparting such facts as the teacher had in mind. Four editions of this classic work were given to the public. This is not the place to review its presentations; however, there are excerpts which may be noticed. Opposite the table of contents appears a page addressed to the reader, which begins:

"It may be proper to mention, that in treating of the reaction between particles, or masses of matter, as the ultimate cause, *agreeably to the laws of the Creator*, of the phenomena, and operations of the physical world, and as the trunk, of which repulsion and attraction are the branches, my plan is peculiar. I have adopted this course, because it enables me to give definitions of natural philosophy, chemistry and physiology, which appear to me brief and appropriate.

I subjoin the following definitions from some of the most eminent chemists.

Thomson defined chemistry to be "the science which treats of those events or changes, in natural bodies, which are not accompanied by sensible motions."

According to Henry, "it may be defined, the science which investigates the composition of material substances, and



the permanent changes of constitution, which their mutual actions produce."

According to Murray, "it is the science which investigates the combinations of matter, and the laws of those general forces, by which, their combinations are established and subverted."

Brande alleges "that it is the object of chemistry, to investigate all changes in the constitution of matter, whether effected by heat, mixture, or other means."

According to Ure, "chemistry may be defined as that science, the object of which is, to discover and explain the changes of composition that occur among the integrant and constituent parts of different bodies."

I avail myself of this opportunity to state my reasons, for employing "*reaction*" for "*action*" and "*react*" for "*act*," contrary to general usage, in describing chemical phenomena. It appears to me, that in all cases where chemical action is said to exist, there is really a reciprocal action. Thus nitric acid is said to act upon tin, although it might with at least as much propriety, be said, that tin acts upon nitric acid; since the latter is decomposed by the former. In this case, I would say, that there is a reaction between tin, and nitric acid, or that tin reacts with nitric acid."

The second edition of the "Compendium" appeared in 1834, while the third and fourth editions followed in 1836 and 1840 respectively. In presenting the second edition this prefatory sentence occurs:

"I have little to add to the ideas presented to the reader of the first edition, but the accumulation of new facts makes it necessary to add and alter quite extensively."

Caloric was much discussed. Its influence in the expansion of solids, liquids, and "aeriform fluids" was experimentally shown in much detail. The modification of its

effects by atmospheric pressure was similarly demonstrated. "The quick communication of caloric in radiation" also received ample consideration. There was further a discussion of means of "evolving caloric."

Light, in its refraction, dispersion, in its heating, illuminating and chemical effects, received much consideration. Crystallization, chemical attraction and affinity occupied much space.

The writer was eager to observe how the Daltonian theory was treated by Hare. It will be recalled that in the "Minutes" the laws of definite and multiple proportions were accepted. In the second edition of the "Compendium" occur, under the section on the atomic theory, these words:

"Were atoms chemically divisible, *ad infinitum*, any one substance, however small in quantity, might be diffused, in a state of chemical combination, throughout any other, having an affinity for it, however great, for as no one particle in the latter, would exercise a stronger affinity than another, it would be unreasonable that each should not have its share. That such a diffusion is impracticable must be evident from the smallness of the number of definite proportions to which substances in combining are restricted, as already mentioned in entering upon the subject of equivalents. Hence elementary atoms are not considered as liable to an unlimited subdivision, either by chemical or mechanical agency.

The ratios of the equivalent numbers are supposed to be dependent on, and identical with, those of the integrant atoms of the substances to which they appertain. Thus the fact that 32 parts, by weight, of soda ( $24 + 8$ ), will saturate as much of any acid, as 48 parts, of potash, is explained by supposing that the weights of the smallest atoms, of those alkalis which exist, are to each other as 32 to 48.



In like manner it is explained that when neutral salts are made reciprocally to decompose each other, no excess, of either ingredient, is in any case observable. The lime, in nitrate of lime, is to the potash, in an equivalent weight of the sulphate of potash, as 28 to 48, yet neither is the lime incompetent to take the place of the potash, nor is there too much potash to take the place of the lime. The result is intelligible, if we suppose, that when quantities, just adequate, for reciprocal decomposition, are employed, there is an equal number of atoms, of each salt; the one containing as many atoms of potash, weighing 48, as the other contains atoms of lime weighing 28.

The same explanation is also applied to explain the fact that while the sulphuric acid in the sulphate of potash is to the nitric acid in the nitrate of lime as 48 to 54, yet neither is there too much of the latter nor too little of the former, to produce neutral compounds with the bases to which they are severally transferred.

On account of the hypothetical association of the numbers, representing the least proportions in which bodies are known to combine, with the supposed relative weight of their atoms, those numbers are as well known by the appellation of atomic weights, as that of chemical equivalence."

The list of elementary substances is placed at fifty-four.

Glucinium and columbium appear. Symbols are not given in the table of names of the elements. These first appear in the third edition, and in connection therewith Hare remarked: "In obedience to the example of the British chemists, I employ Po and So, instead of K and Na, as the symbols of potassium and sodium."

Not to encumber the text, but as a matter of curiosity and to refresh the memory as to the aspect of a few things chemical in 1840, the table, as it appears in the fourth edition is here given:

	Symbol	Atomic Weight		Symbol	Atomic Weight
Aluminium	Al	14	Silver	Ag	108
Antimony	Sb	64	Sodium	So	24
Arsenic	As	38	Strontium	Sr	44
Barium	Ba	38	Sulphur	S	16
Bismuth	Bi	71	Tellurium	Te	64
Boron	B	11	Tin	Sn	59
Bromine	Br	78	Zinc	Zn	32
Calcium	Ca	20	Cadmium	Cd	56
Carbon	C	6	Cerium	Ce	46
Chlorine	Cl	36	Chromium	Cr	28
Copper	Cu	32	Cobalt	Co	30
Fluorine	F	18	Columbium	Ta	185
Gold	Au	200	Glucinium	G	18
Hydrogen	H	1	Iridium	Ir	99
Iodine	I	126	Manganese	Mn	28
Iron	Fe	28	Molybdenum	Mo	48
Lead	Pb	104	Nickel	Ni	30
Lithium	L	6	Osmium	Os	100
Magnesium	Mg	12	Palladium	Pd	53
Mercury	Hg	202	Rhodium	R	52
Nitrogen	N	14	Thorium	Th	60
Oxygen	O	8	Titanium	Ti	24
Phosphorus	P	16	Tungsten	W	95
Platinum	Pl	99	Uranium	U	217
Potassium	Po	40	Vanadium	V	69
Selenium	Se	40	Yttrium	Y	32
Silicon	Si	8	Zirconion	Zr	34

To-day we appreciate the law of Dulong and Petit, and as to that of Faraday, how would the electro-chemist fare were he without it? Hence the attitude of Hare, years ago, to these fundamental deductions cannot fail to arouse a bit of curiosity. So we eagerly read:

"It appears from some experiments made by Messrs Petit and Dulong, that the capacities for heat, or specific heats, of all elementary atoms are the same; so that if the specific heat of any one congeries of atoms be less than that of another having the same weight, it is because the atoms



of the one being heavier than those of the other, there are fewer of them in the same weight. Hence the capacities, or specific heats, of equal volumes of elementary substances are greater, as the weights of their atoms are less; so that if, in the case of each, its atomic weight be multiplied by its specific heat, the product will in general be so nearly the same, that the difference may be ascribed to the inaccuracy unavoidable in experimental investigations.

Respecting this highly important and interesting inference of Petit and Dulong, Alexander Dallas Bache has endeavored to show in an article published in the *Journal of the Academy of Natural Sciences*, that multiplying the equivalents of twelve principal metals into their specific heat, gives results so widely deviating from uniformity as to take all plausibility from the hypothesis that the atoms of simple bodies have the same specific heat.

Dr. Thomson has observed that this law is more likely to be true, since it holds good without doubt in the case of the gases; and that if it be true we have only to divide the specific heat of hydrogen by the atomic weight of any body, to find its specific heat. Moreover, that the specific heats thus found agree very nearly with those ascertained experimentally.

From the researches of Faraday, it appears that the quantity of the voltaic fluid given out during the solution of various metals, is in ratio of their atomic weights. It would seem, therefore, as if the imponderable atmospheres, both of caloric and electricity, are held by atoms in the same equivalent proportion."

Caloric, light and electricity were the agents to which Hare was constantly exposing chemical substances. To him the galvanic current was the power which dominated. There is, therefore, abundant excuse for the relatively large considerations accorded these forces in his text. It is most profusely illustrated. The numerous forms of apparatus were

practically made by Hare's own hands. Some of them were of gigantic proportions and his evident purpose was to perform all experiments upon a grand scale. A test-tube experiment did not satisfy his views; it must be striking—imposing, if you please. This is pardonable in every way, and it must not be forgotten that his student audiences, scattered throughout a large lecture room, were really very large. They did not retire after lectures to laboratories and there verify many of the facts laid before them.

His mode of presentation of his subject matter is plainly indicated in the following paragraphs:

“ Having in the preceding pages treated of certain general properties of ponderable matter, or those means of ascertaining or observing them of which a knowledge is indispensable to a chemist, I shall, in the next place, proceed to the consideration of ponderable substances individually, and their reactions and combinations with each other.

In treating of ponderable elements and their multifarious compounds, various arrangements have been pursued by different writers. Some have preferred to begin with elements, and to proceed to compounds; others to begin with compounds, and to proceed to elements. In favour of the last mentioned course, it may be alleged, that the most interesting substances in nature become known to us at first, in a state of combination. Thus, for instance, the air, water, salts, acids, alkalies, also flesh, sugar, farina, and other organic products, valuable either as food or as medicine, are compounds which have been naturally made the subjects of chemical inquiry; and it may be inferred that the student might with advantage be induced to travel in those paths, of which a successful pursuit has led to that chemical knowledge which it is the object to impart. In this way he proceeds from facts which he knows, to such as he ought to learn, in the order in which he would spontaneously advance as far as he might



be competent. But it may be objected, that no sooner are the ingredients of a body stated, than the student is distracted by names, of which he is ignorant; and which there is an immediate necessity to explain. Hence it follows that the ingredients of a compound may come to be considered in immediate succession, when they may have no analogy with each other; while it is highly advantageous, after having treated of any one element, to proceed to that which has the greatest analogy with it. In that case, a certain portion of the conceptions which have been formed respecting one element, may be extended to another, with little mental exertion, and without much additional pressure upon the memory.

The method first mentioned of treating of each elementary substance first, and afterwards of compounds, is objectionable, because it cannot be put into practice effectually. To treat of the chemical habitudes of any one element, requires that we should speak of other elements, in reacting with which, those habitudes are displayed, and respecting which a beginner is of course ignorant. In pursuing this course, each substance must be treated of imperfectly, or language and illustrations employed, which the student is unprepared to understand.

The course which I have chosen is as follows: I begin with the element which, of all ponderable matter, has the most important part assigned to it in nature, I mean oxygen. The history, state of existence in nature, means of procuring, and properties of this substance, so far as they can be rendered intelligible to a novice, are stated or exemplified and explained. In the next place to oxygen, I present chlorine to attention, which has at least as much analogy with oxygen, as any other known element, and is at the same time, an agent of high importance. Having treated separately of oxygen and chlorine, as far as may be expedient, the compounds which they form with each other, may in the

next place, to a certain extent, be treated of with advantage. Then, guided by analogy, bromine and iodine, though inferior in importance, may be successively treated of, and subsequently all the compounds which they can form, either with oxygen or chlorine, or with each other. This system will be followed in treating of all the elements.

Pursuant to this method, little can be said of fluorine in the section appropriated to its consideration, since those elements with which its most interesting reactions take place cannot consistently be made the object of attention under that section.

Cyanogen is, in its properties, analogous to chlorine, bromine, and iodine, yet being composed of carbon and nitrogen, should not be an object of attention, until the pupil is prepared by a knowledge of its said constituents. Besides, it comes in consistently under the general head of carbon, which, agreeably to my plan, as above explained, comprises the compounds of carbon with all substances previously treated of, among which is nitrogen. . . .

Of the fifty-four elements, *chlorine*, *bromine*, *iodine* and *fluorine* are classed by Berzelius under the name of *halogen bodies*, or generators of salts; while *oxygen*, *sulphur*, *selenium*, and *tellurium* are classed together under the name of *amphigen bodies*, or both producers; meaning that they are productive both of acids and bases. To the elementary halogen bodies, he adds the compound body cyanogen. I object to this classification, that the word salt admits of no definition, reconcilable with the use which has been made of it by the distinguished author; and because, from facts and definitions practically sanctioned by him, and chemists in general, it is evident that the elements belonging to both of his classes are productive of acids and bases. Hence I have associated them in one class, under the appellation of *basacigen elements*. In honour of Berzelius, I shall, however,



retain the terms halogen and amphigen, in order to designate the elements which he has distinguished by those names. It may be proper to add that we owe to Berzelius himself the idea that any other substance besides oxygen could form acids and bases capable of uniting to form salts. Our knowledge of the existence of this faculty in three of his amphigen elements, sulphur, selenium, and tellurium, is, I believe, entirely due to his investigations. If chemists, myself among others, who consider his double salts as consisting of acids and bases, are in the right, it is to the light afforded by his brilliant discoveries that we owe the ability to pursue the true path.

Before concluding this preliminary exposition of the classification and nomenclature which I propose to adopt, I wish to make it clear, that the attribute of producing both acids and bases, which, agreeably to the plan of Berzelius, is restricted to his four amphigen elements, is, agreeably to mine, extended to the elements comprised in both of his classes, which are consequently united under one designation, as basacigen elements. My basacigen class is, therefore, the amphigen class of Berzelius, enlarged under a new and more descriptive name, so as to take in both of his halogen and amphigen classes.

In order to render the definition of a basacigen body precise, it may be necessary that I should give a definition of acidity and basidity.

And then at considerable length he proceeds to enunciate his views as set forth on p. 221, etc.

Let us hear him speak of oxygen.

“In the gaseous state, oxygen forms one-fifth of the atmosphere in bulk; and as a constituent of water in the ratio of eight parts in nine, it pervades every part of the creation where that important compound is to be found. It exists in that congeries of oxidized matter which we call earth, and is a principal and universal constituent of animal and vegetable

matter. Its combinations with metals and various other combustibles are of the highest importance in the arts. It was called oxygen under the erroneous impression of its being the sole acidifying principle, from the Greek *όξύς* acid, and *γίνομαι* to generate.

It can only be isolated in the form of a gas. It is yielded by red lead, nitre, or black oxide of manganese, when exposed to a bright red heat in an iron bottle. There are various other means of obtaining oxygen gas. It is generally supposed that, in order to obtain it in a high degree of purity, chlorate of potash must be employed; but I have found the first portions of the gas as evolved by a red heat from nitrate of potash or nitrate of soda very nearly pure; and Dr. Thomson alleges that this salt, by exposure to a carefully regulated heat, parts with one-fifth of the oxygen of its acid in a state of purity; or in other words, it gives up an atom of oxygen for every atom of the salt, which is equal to 8 parts of 102 parts, or rather more than one-thirteenth."

While of chlorine he said, "It has a curious property, first noticed by me, I believe, of exciting a sensation of warmth; though a thermometer, immersed in it at the same time, does not indicate that its temperature is greater than that of the adjoining medium. . . . About thirty years ago, chlorine gas was universally considered as a compound of muriatic acid and oxygen. It is now deemed an elementary substance, rendered gaseous by caloric."

In the course of his discussion on chemical subjects, in connection with combustion, Hare remarks:

"I would define combustion to be a state of intense corpuscular reaction, accompanied by an evolution of heat and light.

That increase or diminution of temperature consequent to chemical combination, which constitutes combustion when



productive of heat and light, has been ascribed to a mysterious law, by which bodies undergo a change in their capacity to hold caloric. It has been supposed that the capacity of the compound is in some instances greater, in others less, than the mean capacity of the constituents; and that in the former case union is followed by an absorption of caloric, and of course, by cold; in the latter, by production of heat. Yet, when the capacities of compounds are compared with those of their ingredients, the result does not justify the idea that the heat given out by the latter in combining, is produced by a diminution of capacity. At best, this hypothesis only substitutes one enigma for another; since it does not account for the alleged change of capacity.

The diversity of power to hold caloric in a latent state, technically designated by the word capacity, is now generally ascribed to the intervening influence of electricity. It has been shown that, if neighboring bodies be electrified, by means either of gas or resin, previously subjected to friction, they will repel each other; but that if one be thus excited by glass, and another by resin, attraction between them will ensue. Hence the excitements are considered of an opposite nature. It will be recollected that, according to the Franklinian theory, the vitreous excitement results from a redundancy; the resinous, from a deficiency of the electrical fluid. The former being designated as positive, the latter as negative electricity. Agreeably to the doctrine of Duffay, the different electric excitements are considered as the effects of two different fluids, attractive of each other, but self-repellent. The one has accordingly been called resinous, the other vitreous electricity. Yet, even by electricians, who suppose the existence of two fluids, the terms positive and negative are employed.

It has been suggested that Voltaic phenomena, combustion, acidity, alkalinity, and chemical affinity, may owe their existence to the principle by which the different electric ex-

citements are sustained in electrified bodies, modified in some inexplicable manner, so as to act between atoms instead of masses. This suggestion derives strength from the following facts, which have been fully illustrated in my lectures on electricity and galvanism.

The pole of a Voltaic series, terminated by the more oxidizable metal, has been shown to display a feeble electrical excitement, of the same kind as that which is producible by friction in glass; while the other pole displays the opposite excitement, in like manner producible in resin. From reiterated experimental observation it is now generally inferred, that, of any two elementary atoms, chemically combined, and simultaneously exposed, to the voltaic current, one will go to the positive, the other to the negative pole. Atoms are supposed to have electrical states the opposite of those of the poles at which they may be liberated, and are said to be electro-negative when liberated at the positive pole, or anode; electro-positive when liberated at the negative pole, or cathode.

Substances which have opposite relations to the Voltaic poles, have an affinity for each other, which is usually stronger in proportion as the diversity of their electric habitudes is the more marked. Thus, for instance, oxygen, which is pre-eminently electro-negative, and potassium, which is pre-eminently electro-positive, have under ordinary circumstances, a predominant affinity for each other.

On all sides it must be admitted that between chemical reaction, galvanism, and electro-magnetism, there is an intimate association which must be explained before the phenomena of chemical reaction can be well understood.

It has been mentioned that, of known bodies, oxygen appears to be the most electro-negative. It is questionable whether the grade next to oxygen, in the electro-negative scale, is to be assigned to chlorine or fluorine. After these follow bromine, iodine, sulphur, selenium, and tellurium.



Among the metals we have a series of substances, varying from those in which the electro-positive power is pre-eminently great, as in potassium, sodium, lithium, barium, calcium, magnesium, &c., to such metals as belong rather to the electro-negative class. Hence, setting out from the extreme above mentioned, we may proceed through a long range of metals less and less electro-positive, till we arrive at such as produce electro-negative combinations with oxygen or chlorine, or both. More or less within this predicament, I think we find tin, mercury, gold, platinum, palladium, antimony, arsenic, molybdenum, and lastly tellurium. Thus at the intermediate point between the extremes at which oxygen and the alkalifiable metals are placed, there are substances whose relation to the Voltaic poles is equivocal or wavering; and it should be understood that this relation is always comparative. Chlorine is electro-positive with oxygen and perhaps fluorine, and electro-negative with every other body. Iodine is electro-positive with oxygen, chlorine, bromine, and probably fluorine, while with other substances it is electro-negative.

Substances of the two opposite classes, in combining with each other, constitute compounds which are either electro-positive or electro-negative, accordingly as the different energies of their ingredients preponderate. Thus in alkalies, consisting of oxygen united with the alkalifiable metals, the electro-positive influence predominates; while the reverse is true of acids, consisting of the same electro-negative principle, oxygen, in combination with sulphur, nitrogen, phosphorus, carbon, boron, silicon, selenium, or other substances, which in their electrical habitudes, lie between oxygen and those metals.

In some cases we see an electro-negative or electro-positive power attached to compounds, which is not equally displayed by either of their constituent elements separately. Cyanogen, consisting of carbon and nitrogen, is a striking instance of an electro-negative compound thus constituted;

and in ammonia, and the vegetable alkalies lately discovered, we have instances of electro-positive compounds, produced from principles comparatively electro-negative.

For any further view of the connexion between chemical and galvanic reaction, I refer to my Treatise on Galvanism, or Voltaic Electricity.

OF THE INFLUENCE ON CLASSIFICATION AND NOMENCLATURE  
OF THE HABITUDES OF CHEMICAL AGENTS WITH THE  
VOLTAIC SERIES.

It would follow from the statements made under the last head, that there should be a resemblance between the properties of substances which have a proximity to each other, in the electric series. Accordingly we find, that those which occupy the higher part of the electro-negative scale, have, by distinguished writers, especially in Great Britain, been classed as *supporters*; while those which are electro-positive, or feebly electro-negative, have been by the same authors classed as *combustibles*. Also, certain electro-negative compounds, formed of the pre-eminently electro-negative principles, have been associated as *acids*; while other compounds, of oxygen at least, which have the opposite polarity, have been associated as *bases*, under some of the subordinate divisions of alkalies, alkaline earths, earths proper, or simply oxides.

The idea of a class of supporters of combustion, and of combustibles, has no better foundation than that certain substances are the most frequent agents in combustion. Thus hydrogen will produce fire with oxygen and chlorine only; sulphur with oxygen, chlorine, and the metals; and carbon with oxygen; but as either oxygen or chlorine will burn with a greater variety of substances, they have been called supporters of combustion, and the substances with which they combine during the combustion, combustibles. Iodine and latterly bromine have been classed among the supporters; because they combine with almost all the bodies with which



the other elements classed under the name unite, and in some cases with an evolution of heat and light. Yet they are not gaseous like oxygen and chlorine, and are as analogous to sulphur as to oxygen. There appears to me to be an error in taking either of these substances into the class of supporters, while sulphur is excluded, which, next to oxygen and chlorine, has the property of burning with the greatest number of substances. In other respects sulphur seems, in its properties, to be intermediate between iodine and phosphorus. The habitudes of selenium appear to range between those of tellurium and sulphur.

Hydrogen, phosphorus, carbon, boron, silicon are no more entitled to be called combustibles, than oxygen, chlorine, bromine, and iodine, &c., to be called supporters. It should be observed, also, that these appellations are evidently commutable according to circumstances; since a jet of oxygen, fired in hydrogen, is productive of a flame, similar to the inflamed jet of hydrogen on oxygen. If we breathed in an atmosphere of hydrogen, oxygen would be considered as inflammable, and of course a combustible. The arrangement which I have adopted of classifying as basacigen bodies, those which have heretofore been treated as supporters, with the addition of some others, renders it unnecessary to resort to the incorrect division into supporters and combustible.

METHOD OF DISTINGUISHING DEGREES OF OXIDIZEMENT,  
DERIVED FROM THE SCHOOL OF LAVOISIER.

The method which, in concurrence with Thênard, I have pursued in designating in the case of the compounds formed by the basacigen bodies with radicals, the proportion of the former ingredient has been stated.

In the case of oxacids another method was adopted by the Lavoisierian School, which, with some modification, still endures, and which I shall state as it now prevails.

Agreeably to the nomenclature in question, where, in

consequence of different degrees of oxidizement substances form two acids, one containing a larger, the other a lesser proportion of oxygen, the acid, having the lesser proportion, is distinguished by the name of the substance oxygenated, and a termination in *ous*; as sulphurous acid and sulphuric acid. That ingredient in an acid or a base, which is least electro-negative, is called the radical. When an acid is discovered having less oxygen than one with the same radical of which the name ends with *ous*, the word *hypo* is prefixed. Hence the appellations, *hyponitrous*, *hyposulphurous*. The same means of distinction is employed to designate a degree of oxygenation exceeding that designated by *ous*, but less than that designated by *ic*. Hence the name *hyposulphuric*. If there be an acid having still more oxygen than the one of which the name ends in *ic*, the letters *oxy* are prefixed.

Acids of which the names terminate in *ous*, have their salts distinguished by a termination in *ite*. Acids of which the names end in *ic*, have their salts distinguished by a termination in *ate*. Thus we have *nitrites* and *nitrates*, *sulphites* and *sulphates*. If the base be in excess, the word *sub* is prefixed, as *subsulphate*. If the acid be in excess, *super* is prefixed, as *supersulphate*. The letters *bi* are placed before the name of salts having a double proportion of acid; hence *carbonate* and *bicarbonate*.

The oxide in which the oxidizement is supposed to be at the maximum is called the *peroxide*. This monosyllable, *per*, is also used in the case of acids, to signify the highest state of oxygenation, and has been substituted for *oxy* in the case of *perchloric acid*. Many chemists apply the monosyllable in question to distinguish a salt formed with a peroxide. Thus the red sulphate of iron has been called the *persulphate* of iron; the nitrate of the red oxide of mercury, the *per-nitrate* of mercury. Agreeably to a similar rule, salts formed with *protoxides* have the word *proto* prefixed; as in the instances of *protonitrate*, *protosulphate*, &c.



It has already been stated that by the British chemists the binary compounds of oxygen, chlorine, bromine, iodine, fluorine, and cyanogen, when not acid, are designated by the termination in *ide*.

The word oxide has been erroneously used as a correlative of the word acid, instead of being used as a generic name for any compound of oxygen, whether an acid or base. I should deem it preferable to apply the termination in *ide*, to all compounds of the basacigen bodies, whether acids, bases or neutral, employing the words acid and base as terminations to indicate the subordinate electro-negative, and electro-positive compounds. In that case *oxybase*, *chloribase*, *fluobase*, *bromibase*, *iodobase*, *cyanobase*, *sulphobase*, *selenibase*, *telluribase*, would stand in opposition to *oxacid*, *chloracid*, *bromacid*, *iodacid*, *cyanacid*, *sulphacid*, *selenacid*, *telluracid*. Yet for convenience, the generic termination *ide* might be used without any misunderstanding; and so far the prevailing practice might remain unchanged. Resort to either appellation would not, agreeably to custom, be necessary in speaking of salts or other compounds analogous to them; since it is deemed sufficient to mention the radical, as if the salt consisted of an acid combined with a radical, not an oxide. Ordinarily we say sulphate of lead, not sulphate of the oxide of lead. This last mentioned expression is resorted to, only where great precision is desirable. In such cases, it might be better to say sulphate of the oxybase of lead.

The method of indicating the proportion of oxygen in an oxide, by changing the termination from *ous* to *ic*, has been generally adopted only in the case of the protoxide, and bioxide of nitrogen, the former being usually called nitrous oxide, the latter nitric oxide. In the Berzelian nomenclature, this method of discrimination has been extended to all the compounds formed with amphigen and halogen elements. Hence we have chlorure mercureux, and chlorure mercurique,

for the protochloride, and bichloride of mercury; and again, oxide mercureux and oxide mercurique for the protoxide and bioxide of the same metal. These Berzelian names translated into English would make mercurious chloride and mercuric chloride, mercurious oxide and mercuric oxide.

It should be understood that the employment of the terminations in *eux* and *ique*, which in French answer for *ic* and *ous* in English, is extended, by Berzelius, to the case of all oxides, whether acids or bases. These words are, in my opinion, neither agreeable to the ear, nor sufficiently definite and descriptive. In the received nomenclature, besides the case above cited of the bioxide of nitrogen, the only other instance, of the employment of the letters *ic* to designate an oxide, is that of the protoxide of carbon, called carbonic oxide.

#### OF THE ORIGIN OF THE ERRONEOUS IDEA OF A PONDERABLE ACIDIFYING PRINCIPLE.

At the period when the French nomenclature was adopted, oxygen was considered as the sole *acidifying principle*, whence its name as already stated. Of course, every acid being supposed to consist of oxygen in part, it was enough to call it an acid to convey a correct idea of its composition in that respect. But when, at a subsequent period, it was shown that many acids were destitute of oxygen, and that other substances were nearly as efficient as oxygen in generating acids by a union with acidifiable bodies, it became necessary to prefix syllables in order to distinguish the acid compounds produced by one acidifying principle, from those produced by others. The term acidifying principle originated with the error of assigning that character exclusively to oxygen. From convenience, more than any conviction of its propriety, it was afterwards used occasionally in reference to chlorine, hydrogen, and other elements which are found to produce acids by combining with a variety of substances. It must be obvious that there is no adequate reason for considering any ponderable element as an acidifying principle.



Subsequently to the creation of the word oxygen, the word radical was employed to designate an oxidizable substance. It has since been extended by me to all substances which form acids or bases with the *basacigen bodies*.

#### OF ACIDITY

Acidity and sourness were originally synonymous. By some of the older chemists, the solvent power of certain acid or sour liquids, was ascribed to the sharpness of their constituent particles. To this acuteness in form, the power of penetrating and severing the combinations of other particles was attributed. With people in general, the words acid, and acidity, still retain their original signification; but by modern chemists, substances are associated as acids which are destitute of sourness, and which are extremely discordant in their obvious properties. Thus we have in the group of acids, sulphuric acid, and flint, vinegar and the tanning principle; also the volatile and odoriferous liquid, called prussic acid, and the unctuous, insoluble, inert, concrete material for candles, called margaric acid. It might naturally excite the curiosity of the learner, to know by what common characteristic substances so discordant had been affiliated. It would be inferred that there should be some test of acidity, by which to determine whether a new compound should belong to the class of acids or not. I am utterly ignorant of any other common characteristic, in these otherwise heterogeneous substances, besides that common preference for the poles, or *electrodes*, of the Voltaic series, on which I have founded my definition of acidity and basidity; coupled with the inference, mentioned in a note, that any compound capable of neutralizing a base, is deemed to be an acid; and *vice versa*, any compound capable of neutralizing an acid, is deemed to be a base. To me it is quite evident that it is only upon one or the other of these characteristics, that many organic compounds which are called acids, or bases, can have any pretensions to be designated as they are.

Among the characteristics of acidity heretofore relied on, is that of reddening vegetable blues. By the soluble acids, this property is generally possessed, although an aqueous solution of sulphurous acid is said to whiten litmus; the vegetable blue is generally employed as a test of acidity.—But indigo is not reddened by any acid, although by nitric acid it is destroyed. Solubility, though usually a property of acids, is in many cases wanting, as in those of margaric and stearic acid, and others of similar origin. The acid properties of silicic, and boric, acid, are displayed at temperatures incompatible with any other solubility, than that which is effected by the agency of caloric.

#### OF ALKALINITY

Among the metallic oxides which, agreeably to the definitions above given, are considered as bases, there are a certain number which are called alkalies, on account of some peculiarities which I shall proceed to mention.

All the alkalies have a peculiar taste, called alkaline. They all produce, in certain vegetable colours, characteristic changes, which differ according to the matter subjected to them, but are not varied by changing the alkali.

They restore colours changed by acids, and are capable of neutralizing acidity.

Acids neutralize alkalies, and restore colours destroyed by them. Acids do not usually combine with acids, nor alkalies with alkalies, but acids and alkalies unite energetically with each other.

By the reaction of alkalies with oils, soaps are generated, which are soluble in water.

Besides the alkalies above named, there are four other metallic oxides, those of magnesium, barium, and strontium, for instance, which have been called earths, and which, in different degrees of intensity, have all the alkaline properties above mentioned, excepting that, if not insoluble, they have



an inferior solubility and that they do not form soluble soaps.

There are also some vegetable compounds which possess, to a sufficient extent, the attributes of alkalies, to be classed among them.

According to Bonsdorff, the halogen elements of Berzelius produce bases, which in some cases display alkalinity. He has noticed a change of colour, indicating an alkaline reaction, on litmus paper, reddened previously by an acid, and dipped into a solution of a chloride, either of calcium, magnesium, or zinc.

I infer that acidity, basidity, alkalinity, and galvanic polarity, are due to some inscrutable influence of the imponderable cause, or causes, or heat, light, and electricity. To a like influence I ascribe the sweetness of sugar, the pungency of mustard or pepper, and of essential oils, as well as the endless variety of odour with which these last mentioned products are endowed. It is evident that in the organic alkalies and acids, alkalinity and acidity are found to be associated with combinations of ponderable elementary atoms, which exist in other combinations without inducing alkalinity or acidity."

And he further commented on the nomenclature of the cyanogen compounds.

The union of hydrogen fluoride with either boron fluoride or silicon fluoride is discussed as follows by Hare:

"The union which ensues between fluohydric acid, and either fluoboric, or fluosilicic acid, agreeably to the preceding statement, may appear anomalous, in the way in which it has hitherto been treated. If, however, I am correct in my mode of defining the difference between an acid and a base, the combinations in question will not prove to be anomalous. I deem it consistent to suppose that a fluobase of hydrogen united, in the one case, with fluoboric acid, in the other with fluosilicic acid; so that fluohydroboric acid might be called

fluoborate of the fluobase of hydrogen, or more briefly fluoborate of hydrogen; and in like manner, fluohydrosilicic acid would be called fluosilicate of the fluobase of hydrogen, or briefly fluosilicate of hydrogen.

When either fluohydroboric acid, or fluohydrosilicic acid, or in other words the fluoborate or fluosilicate of the fluobase of hydrogen, is brought into contact with an oxybase, the radical of the latter takes the place of the hydrogen, which, with its oxygen, forms water. Thus, in the case of potash, there would result a fluobase of potassium, usurping the place of the fluobase of hydrogen; and of course either a fluosilicate, or fluoborate of potassium must be formed. Agreeably to the Berzelian nomenclature, these compounds are double salts, the name of one being in the French translation, "*fluorure borico-potassique*," that of the other, "*fluorure silico-potassique*." Many analogous salts, formed by the acids under consideration, with salifiable substances, are mentioned by Berzelius; also many others, in which other radicals, in union with fluorine, play a part analogous to that performed by silicon and boron, in the salts above mentioned.

There are instances in which compounds, usually called bases, act as acids. Of course, it is consistent that compounds, usually called acids, should in some instances act as bases. In this respect, a striking analogy may be observed between the union of the oxide of hydrogen (water) with the oxacids and oxybases; and that of fluoride of hydrogen with fluacids and fluobases. According to Berzelius, water acts as a base of oxacids; as an acid to oxybases. So I conceive the fluoride of hydrogen acts as a base in the cases above noticed, while it acts as an acid in the compound of hydrogen, fluorine, and potassium, called by Berzelius "*fluorure potassique acide*." This compound I would call a fluohydrate of the fluobase of potassium, or more briefly, fluohydrate of potassium; as we say sulphate of copper, instead of the sulphate of the oxide (or oxybase) of copper. . . ."



Salts were described in much detail. And, in the pages which precede there is quite a bit of evidence of the views entertained by Hare on topics in the inorganic field. It will be most interesting to follow him in "Organic Chemistry"—"the chemistry of organic substances." In the introduction, among others, occur these sentences:

"It is generally a marked distinction, between organic and inorganic products, that the latter can, in a much greater number of instances, be imitated by art. The incompetency of chemists to regenerate the substances analyzed by them, has caused the accuracy of their deductions to be questioned. Rousseau having heard Rouelle lecture on farinaceous matter, said he would not confide in any analysis of it, till corroborated by its reproduction from the elements with which it was alleged to have been resolved. . . ."

What would that savant say could he now behold the synthetic conquests in the organic domain, *e.g.*, that of indigo, alizarin, etc., etc.?

"At first view it may seem reasonable to consider synthesis as the only satisfactory test of the truth of analysis. But if when diamond is burned in one bell glass and charcoal in another, in different portions of the same oxygen gas, and subsequently in each vessel, in lieu of the diamond and charcoal, carbonic acid is found, from which, by potassium, carbon may be liberated, who would hesitate to admit both substances to consist of carbon, because this element cannot be recovered in its crystalline form from the gaseous state?"

And, in adverting to formulæ which then probably were much discussed, Hare said:

"As with very few exceptions in formulæ expressing the composition of organic substances, only four different letters are requisite, with the figures showing the relative proportions, the employment of symbols for that purpose is evidently highly advantageous. The student, therefore, is ad-

vised especially to overcome, by a proper degree of resolution, any repugnance to the study of the formulæ given, or others which may be resorted to in this or in other modern treatises of chemistry. A comparison of their formulæ, respectively, will convey an idea of the difference in composition existing between the radicals in the preceding list. . . .”

And he continues: “I adverted to the fact that certain elements may be substituted, the one for the other, without changing the crystalline form. Dumas has latterly held an analogous doctrine respecting the substitution, in organic products, of one element for another, or of a compound radical for an element, without “*altering the general chemical type*,” as he calls it; and would have the bodies thus formed grouped together, constituting a natural family.

Hare’s comments on radicals are original and characteristic.

Liebig alleged, that “reciprocal substitution of simple or compound bodies, acting in the manner of isomorphous bodies, should be considered as a true law of nature.” To this Hare replied: “This substitution may take place between bodies which have neither the same form, nor any analogy in composition. It depends exclusively on the chemical force, which we call affinity.”

In consonance with the law in question, Dumas has found, that in acetic acid chlorine may be substituted for hydrogen, and that in this way a new acid, designated as chloroacetic, may be produced.

This chloroacetic acid is by him alleged to be, in its properties, so analogous to acetic acid, that to know the habitudes of the one, conveys an idea of those of the other. This analogy he conceives to arise from a chemical law, agreeably to which the properties of a compound depend rather on the type of the composition, than on the particular character of the elements which may have been exchanged.



Of saponification he says: "Anterior to the labours of Chevreul, an erroneous notion existed that the process of saponification consisted in nothing more than a union between the alkali and oil; so that it was deemed to be a case simply of combination. The existence in every oil of an electro-negative, and an electro-positive ingredient, the one performing the part of a base, the other of an acid, was not imagined."

And of sugar he wrote:

"As sugar has been found to be very susceptible of yielding alcohol by fermentation, this property has been made the basis of defining the meaning of the word, so that every substance capable of the process alluded to, is to be considered as sugar, whatever may be its taste, or however it may differ in its properties from the substance usually called by the name.

Thus the fermentable '*wort*' of distillers or brewers, the uncrystallizable juices of fruits, a substance found in mushrooms or ergot, also an insipid matter found by Thénard in diabetic urine, are all to be considered as consisting of sugar, so far as they are capable of yielding alcohol by fermentation."

And then he proceeds:

"I am reluctant to employ words in a sense different from that in which they are generally understood. Agreeably to usual acceptation, sweetness is an indispensable attribute of sugar. *Sugary* and *sweet* are synonymous. "*As sweet as sugar*" has long been an expression conveying the idea of superlative sweetness.

But chemists have erred, I think, in assuming that nothing besides sugar is susceptible of the vinous fermentation. The conversion into alcohol of the insipid product of diabetes, which has been treated as sugar, because proved to be susceptible of the process in question, might with more propriety, as I conceive, be deemed to demonstrate that this process may be undergone by substances which are not sufficiently of a saccharine nature to merit the name of sugar. . . .

It is well known to those who are acquainted with the manufacture of whiskey from grain, that a portion of malt is necessary to render the wash or wort susceptible of the vinous fermentation; and that the product is much affected by the circumstances under which the infusion of the grain is accomplished. Nearly thirty years ago, my late friend, Col. Anderson, who had distinguished himself by his ingenuity and sagacity in improving the processes and apparatus of our American distilleries, expressed to me an opinion, that the mixture of farina and water became sweeter towards the close of the process of infusion, and that he believed a chemical change was induced, by which more or less sugar was generated. The inference of our ingenious countryman has been fully justified by the researches of Payen and Persoz, whence it appears that, by digestion with malt, fecula is at first partially changed into a sweetish gummy matter, called dextrine, and that this matter is afterwards converted into grape sugar. *Dextrine* has received its name from a peculiar influence which it exercises upon the plane of polarization, during the passage of light. It may be considered as holding, as respects its properties, an intermediate position between fecula and grape sugar."

In commenting on malic acid Hare adds:

"Professor Wm. Rogers, of the University of Virginia, has ascertained that this acid abounds in different species of sumach, in the state of bimalate of lime. Malic acid is bibasic, its formula being  $C_5H_4O_8 + 2HO$ .

Malic and citric acids afford very good examples of the operation of a law, to which a great many of the vegetable acids are subjected. At a temperature a little above that at which they melt, they severally yield new acids. That yielded by citric acid, is identical with the acid found in the *aconitum napellus*, and also the various species of *equisitum*. Hence, it has received the name of aconitic or equisitic acid. Whether



obtained from citric acid by heat, or from either of its other sources, it exists in the form of white crystals, soluble in water, and sour in taste. The acid into which malic acid is changed, under similar circumstances, is also found in nature in Iceland moss, and in the *fumaria officinalis*. Hence it has been called fumaric acid, although Pelouze, who first obtained it from malic acid by heat, called it *parmalic acid*. Both of these acids differ from the citric and malic acid, from which they are produced, only in having lost the elements of two atoms of water.

When either of the acids thus obtained, by heating citric or malic acid, is exposed to a higher temperature, a further change takes place, and volatile acids are formed, fumaric acid yielding maleic, and aconitic producing itaconic acid. The former would seem to be formed by a mere transposition of the elements of water present, which appear as two atoms of water of crystallization, instead of entering as before as two basic atoms into the integral composition of the acid. A further application of heat converts *itaconic* into *citraconic* acid; while maleic acid, if kept in a state of fusion for a length of time, reverts to the condition of fumaric acid.

It must be observed, that if citric or malic acid be heated, without keeping them at the temperatures necessary for the formation of the acid compounds which they respectively produce, the result will be a mixture in the one case of fumaric acid and maleic acid, in the other, of aconitic, itaconic and citraconic acids."

And in this manner he goes forward in the careful presentation of his subject, injecting as he has opportunity, results won in his own laboratory. For example, in speaking of perchloric ether, he remarks:

"This ether was discovered, in my laboratory, by Mr. Martin Boyé and Mr. Clark Hare.

It was obtained by subjecting about ninety grains of crystallized sulphovinate of baryta, with an equivalent proportion of perchlorate of baryta, to the distillatory process, receiving the product in from one to two drachms of absolute alcohol. By complex affinity, the sulphuric acid of the sulphovinate dispossesses the perchloric acid of the baryta, while, at the same time, the last mentioned acid combines with the oxide of ethyl.

The perchlorate of ethyl is a transparent, colourless liquid, possessing a peculiar, though agreeable smell, a very sweet taste, which on subsiding, leaves a biting impression on the tongue, resembling that of the oil of cinnamon, but more acrid and enduring. It is heavier than water, through which it rapidly sinks. It explodes by ignition, friction, or percussion, and sometimes without any assignable cause. Its explosive properties may be safely shown, by pouring a small portion of the alcoholic solution into a small porcelain capsule, and adding an equal volume of water. The ether will collect in a drop at the bottom, and may be subsequently separated by pouring off the greater part of the water, and throwing the rest on a moistened filter, supported by a wire. After the water has drained off, the drop of ether remaining at the bottom of the filter may be exploded either by approaching it to an ignited body, or by the blow of a hammer. The violence and readiness with which this ether explodes is not surpassed by that of any other known compound. By the smallest drop, an open porcelain plate may be reduced into fragments, and by a larger quantity, to powder. In consequence of the force with which it projects the minute fragments of any containing vessel in which it explodes, it is necessary that the operator should wear gloves, and a close mask, furnished with thick glass-plates at the apertures for the eyes, and perform his manipulations with the intervention of a movable wooden screen.

In common with other ethers, the perchlorate of ethyl



is insoluble in water, but soluble in alcohol; and its solution in the latter, when sufficiently dilute, burns entirely away with explosion. It may be kept for a length of time unchanged, even when in contact with water; but the addition of this fluid, when employed to precipitate it from its alcoholic solution, causes it partially to be decomposed. Potassa, dissolved in alcohol, and added to the alcoholic solution, produces immediately, an abundant precipitate of the perchlorate of that base, and, when added in sufficient quantity, decomposes the ether entirely.

The perchlorate of ethyl has been subjected to the heat of boiling water without explosion or ebullition.

It may be observed that this is the first ether formed by the combination of an inorganic acid containing more than three atoms of oxygen with the oxide of ethule, and that the chlorine and oxygen in the whole compound are just sufficient to form chlorohydric acid, water and carbonic oxide with the hydrogen and carbon. It is also the only ether which is explosive per se."

Ethers and aldehydes are quite fully treated by Hare in accordance with the prevailing views. And, after presenting the following statement from Liebig to the effect that: "The reaction which nitric acid exercises with the hydrated oxide of methyl, is not like that which it exercises with alcohol, since, while this liquid is decomposed with great difficulty, giving birth to certain oxidized products and hyponitrite of the oxyde of ethyl, the hydrated oxide of methyl is not altered by nitric acid, unless at a boiling heat. When a great excess of this acid is employed, formic and oxalic acids are generated, but no hyponitrite ("nitrite") or nitrate of the oxide of methyl. It would seem, therefore, that the *hyponitrite* of the oxide of methyl does not exist. . . ."

He said: "I found however that by subjecting pure wood spirit to the process already described for producing the hypo-

nitrite of ethyl, a congenerous ethereal product was obtained (p. 308). Hyponitrite of methyl has a great resemblance to its congener above named, in colour, smell, and taste; though there is still a diversity sufficient to enable a careful observer to distinguish one from the other.

When the process in which hyponitrous ether is generated, by introducing the refrigerated materials into a bottle surrounded by ice and water, was resorted to, substituting wood spirit for alcohol, it was found that the ether did not separate from the spirit as completely as in the process in which alcohol was the material. This I ascribe to the affinity between water and wood spirit being inferior to that between this mentioned liquid and alcohol. The boiling point of both of the ethers seemed to be nearly the same, and in both, in consequence of the escape of an ethereal gas, an effervescence resembling that of ebullition, was observed to take place at a lower temperature than that at which the boiling point became stationary.

From the language of Liebig above quoted, I infer that previous efforts to produce the methylic hyponitrous ether had failed. The failure of others, and my success, cannot excite surprise, when the difference of the habitudes of wood spirit and alcohol, with nitric acid and alcohol, are taken into view, and the difference between my process and those followed in Europe, by which more or less nitric acid is brought into contact with the spirit employed. When alcohol is presented to nitric acid, a reciprocal decomposition ensues. The acid loses two atoms of oxygen, which, by taking two atoms of hydrogen from a portion of the alcohol, transforms it into aldehyde, while the hyponitrous acid resulting inevitably from the partial deoxidizement of the nitric acid, unites with the base of the remaining part of the alcohol. But when pyroxylic spirit is presented to nitric acid, this acid, without decomposition, combines with methyl, the base of this hy-



drate; hence, as no hyponitrous acid is evolved, no hyponitrite can be produced. Thus in the case of the one there can be no ethereal hyponitrite, in that of the other no ethereal nitrate."

In regard to respiration, Hare said:

"I subjoin an article which I had prepared on respiration, as it contains some ideas which are not found elsewhere, and some objections to Liebig's explanation of the phenomena of that process.

Chemistry demonstrates, that during this process, the volume of the air respired by animals is diminished, but that a portion of the oxygen is replaced by an equal bulk of carbonic acid. Although, at one time, by respectable observers, the volume of this last mentioned gas was alleged not to be uniformly equal to that of the absorbed oxygen, the ratio of the one to the other being represented as varying with the time of day and the season, not only in different animals, but also in the same animal, later observation seems to have produced a general opinion, which is zealously espoused by the distinguished chemist above mentioned, that the expired carbonic acid is, upon the whole, exactly equivalent to the oxygen consumed.

The prevalence of nitrogen, in animal substances, naturally led to the idea that it might be assimilated more or less during respiration; but experience has led to an opposite opinion; and Liebig has endeavoured to show, that in the nutriment of granivorous animals, there is no deficiency of vegeto-animal matter having as large a proportion of nitrogen as flesh and blood.

When first, by the Lavoisierian school, the heat of all ordinary fires was shown to be attributable to the union of oxygen with the combustible employed, the idea naturally followed, that respiration being attended by a like union of oxygen with combustible matter, animal heat ought to be

ascribed to this source. Many objections to this explanation of the origin of animal heat were subsequently urged, and, among others, the fact that the heat of the lungs, *the fire place*, is no higher than remoter parts of the animal frame.

To remove this objection, Crawford suggested that the capacity for heat, of arterial blood, being greater than that of venous blood, caloric was taken up by the blood in one state, to be evolved when in the other. This suggestion respecting the relative capacities for heat, of arterial and venous blood, has not been supported by subsequent experience; and another view of the subject has been taken, which renders it quite consistent that the temperature should not be peculiarly high in the lungs.

It is supposed that the blood merely *absorbs* oxygen in the lungs, but that this oxygen is carbonized during its circulation, and thus causes heat to be given out in all parts of the system. The carbonic acid thus produced, on reaching the lungs in combination with the venous blood, is exchanged for oxygen, and consequently expired with the breath.

Liebig conceives that the iron in the hematosin of the red globules is held by the arterial blood, in the state of hydrated sesquioxide; but in the capillaries, the sesquioxide passing to the state of protoxide, by yielding oxygen to the carbon in the blood, combines with the carbonic acid thus produced, and gives rise, in the venous blood, to a carbonated protoxide.

When the venous blood reaches the lungs, the protoxide exchanging carbonic acid for oxygen, this gas is expelled with the breath, while the regenerated sesquioxide is again, by union with water, reconverted into a hydrate. The well known change of hue which follows the transfer of the blood from the veins to the arteries, through the pulmonary organs, seems to be considered as a collateral consequence of these chemical reactions. Yet this change does not appear to me



sufficiently accounted for, since no such alteration of colour can be produced by the transformation of a carbonated protoxide of iron to a hydrated sesquioxide. Moreover, the fact that no peculiar elevation of temperature takes place on the surfaces where the venous blood meets the breath, seems to me inconsistent with Liebig's explanation, since the heat must be extricated in the space where the iron is peroxidized.

Upon the whole I now think as I have for forty years, whatever other opinions may have prevailed, that there must be a degree of heat derived from respiration proportional to the quantity of oxygen converted into carbonic acid; but with all due deference for Liebig, I do not agree with him, that it is possible to give a satisfactory explanation of this process upon purely chemical affinities, such as exist independently of vital power. It appears to me that nature has the power, within certain limits, of making chemical affinities to suit her own purposes, and can therefore cause the oxygen to be absorbed, the carbon to combine therewith, and the heat to be given out when and where the processes of vitality require it. If nature have not the alleged power, how does it happen that, out of the heterogeneous congeries of elements existing in the egg, the bill, the claws, the feathers, the bones, the blood, and flesh, are made to appear at the various stations, at which their presence is requisite, for the existence of a young bird?

Liebig cites the following interesting facts: An active man expires 13.9 ounces of carbon, and daily consumes, in the same time, 37 ounces of oxygen = 511648 cubic inches, or about 223 gallons. Reckoning 18 inspirations per minute, there must be 25,920 consumed per day, and consequently  $511648/25920 = 1.99$  or nearly two cubic inches of oxygen in each respiration. In one minute, therefore, there are added to the blood 1.99 times 18 = 35.8 cubic inches of oxygen, weighing rather less than twelve grains.

In one minute, ten pounds of blood pass through the lungs, measuring 320 cubic inches, among which 35.8 being divided, there must be one cubic inch of oxygen for nine of blood nearly.

Ten Hessian pounds of blood = 76,800 grains, if in the arterial state, contain 61  $\frac{54}{100}$  grains sesquioxide of iron; if in the venous state, 55  $\frac{14}{100}$  protoxide. 6  $\frac{40}{100}$ , the difference, is the quantity of oxygen which the iron of the venous blood can acquire in the lungs, which deducted from twelve grains, the whole quantity of oxygen absorbed, leaves 5.60 grains requiring some other means of absorption. But 55  $\frac{14}{100}$  grains of protoxide of iron would take up 73 cubic inches of carbonic acid, which is double the volume that the 35  $\frac{8}{100}$  of oxygen can generate.

One glaring defect in this part of the explanation, arises from the admitted fact, that nearly one-half of the absorption of oxygen is unaccounted for; 5.60 in twelve parts."

It is most interesting to note Hare's views on saccharine and vinous fermentation, acetous fermentation, viscous fermentation, particularly as he gave much thought in earlier years to brewing. He wrote:

"I am under the impression that all the four fermentations may ensue either successively, or, to a certain degree, simultaneously. Thus, either starch or lactic may be converted into grape sugar. This product may be partially changed into alcohol, and in part into lactic acid and mannite; while a portion of alcohol simultaneously generated, may be undergoing acetification.

Each fermentation has its appropriate ferment. Thus diastase incites the saccharine fermentation, yeast the alcoholic, oxidized diastase, casein or curd, the lactic; while the scum or sediment, called mother of vinegar, promotes the acetic fermentation. It is the object of the vintner, the brewer, and distiller, to permit only the two first fermenta-



tions, the alcoholic especially, to which the saccharine is accessory. This object is secured by taking great care to have the juice or wort simultaneously subjected to a temperature between  $60^{\circ}$  and  $70^{\circ}$ , and a limited exposure to air, with the addition of the proper ferment, where this is necessary; while, by great cleanliness, the presence of any matter capable of inducing the acetous or lactic fermentation is avoided. Much liquor is spoiled by the substitution of the *viscous* for the *alcoholic* fermentation. . . .

Boutron and Fremy have made some interesting observations respecting the generation of lactic acid in milk. Oxidized caseine is considered by them as pre-eminent in efficacy as a ferment, for the lactic fermentation, by acting on the sugar of milk or lactin; but in consequence of an affinity for the generated acid, the oxidized caseine forms with it an inert compound which precipitates.

The generation of lactic acid requires the presence both of lactin and free oxidized caseine. Of course, in order to increase the production of the acid, it was found necessary to add an additional quantity of lactin to milk, but to renew the efficiency of caseine, it was found sufficient to saturate the lactic acid, as often as the production of this acid was arrested by the precipitation of the oxidized caseine.

Diastase, after being exposed a few days to the air, becomes capable of inducing the viscous or lactic fermentation. The membranes of the stomach of a dog or calf, or the substance of a bladder, by a like exposure, were found capable of inciting the fermentation in question. Yet animal matters, in appearance similarly prepared, are productive of different results, as respects the proportions of mannite, of viscous matter, of lactic acid, or alcohol, generated. The means by which the various ferments, respectively, produce their appropriate changes are involved in the greatest obscurity. The ferments have all been shown to be vegeto-animal matter

in a state of oxidizement, and an analogy seems to have been established between their influence and that of some other agents, which have been considered as acting by what has been called catalysis, which is a new name given by Berzelius to an old mystery. It has long been known that there are two modes by which chemical changes are to be excited. In one of these, the presentation of one or two extraneous elements causes decomposition and recomposition, by the reactions between the elements so presented, and those subjected to alteration, as in the various cases of elective affinity. In the other mode, substances undergo transformations by being made to rearrange their constituents into one or more new combinations, by the presence of other bodies with which they do not combine, and which, in some cases, undergo no change themselves. It is to the last mentioned mode of reaction that the name above mentioned has been applied. Yet, under this head, processes have been crudely associated which have discordant features. Liebig indiscriminately gives a common explanation to these processes, and to those of fermentation, so far as they might be crudely referable to catalysis.

The following processes are associated by this distinguished chemist under one rationale:—*the solubility acquired by platina by being alloyed with silver; the catalyzing influence of platina sponge or platina black; the explosion of fulminating powders by slight causes; the reciprocal decomposition of bioxide of hydrogen and oxide of silver; the agency of nitric oxide in the generation of sulphuric acid; the action of ferments.*

To me it seems that there is a great diversity in the characteristics of the process thus alluded to. In the case of the platina alloy there is at least an atom of silver for each atom of platina in actual combination with this metal; and the change which the latter undergoes is precisely the same as that to which the former is subjected.



In the case of platina sponge, causing the formation of water, or of platina black, causing the acetification of alcoholic vapour, the inducing agent undergoes no change itself; it enters not into chemical combination either with the materials, or the products. Liebig ascribed the result in this instance to the alternate absorption and subsequent evolution of oxygen by the powder; since, after exposure to the gas, it may, by exhaustion, be made to give up a portion. But the agency of this metallic mass cannot differ, in this case, from that in which it causes the pure elements of water to combine, and in which, if absorption take place, it is not confined to oxygen more than to hydrogen. But the fact established by Faraday, that hydrogen and oxygen may be made to unite by a well cleaned plate of platina, seems irreconcilable with the idea that *absorption* is the means of its accomplishment. But if absorption be not operative in one case, how can it operate in the other?

In this, as in all other cases, Liebig seems to overlook the all important agency of electricity in the phenomena of nature. I should infer, that the metal most probably acts by altering the electrical polarity, and consequent association of imponderable matter. But having assumed, that during the dehydrogenation of alcohol by atmospheric oxygen in the presence of platina black, this powder is alternately endowed with the power to take it from the air, and to impart it to that of which the attraction for oxygen, under the circumstances, is too feeble to take it from the same source, this distinguished philosopher proceeds to make the inference that honey, mother of vinegar, and other substances promotive of acetification, act in the same way by absorbing oxygen from the air, and abandoning it to hydrogen. But if agreeably to the view above presented, platina black does not act by absorption, no argument, founded on the agency of that substance, will justify the idea that absorption avails in other

cases; and it should be recollected, that platina black is very active when perfectly free from moisture, while honey, yeast, mother of vinegar, and other substances which cause acetification, have no attraction for oxygen in the absence of water; moreover, that the necessity for moisture to the preparatory oxidizement of gluten, caseine, diastase, and other organic substances, which by exposure in a humid state acquire their capacity to act as ferments, is inexplicable. Water is powerful both as a catalyzer and as a solvent.

Before referring to the absorption of oxygen by honey, as a ground of explanation founded on the analogy of platina black, the ability of water to cause honey to absorb oxygen should be first elucidated.

An electric spark or any ignited body, a wire made incandescent by a galvanic discharge, has an influence analogous to platina sponge, of which the minutest particle is sufficient to cause ignition throughout an inflammable mixture, however large. There is, in this respect, an analogy between the explosion of inflammable gaseous mixtures and those of gun powder, and of other fulminating powders, of which some, as it is well known, detonate by percussion or friction, or any cause adequate to derange the equilibrium of their particles. In the case last mentioned, the change produced is the same, whatever may be the exciting cause, and the minutest portion of the congeries being made to undergo the change, is of itself competent to produce a like result as respects the whole.

The property which bioxide of hydrogen, and the oxide of silver, or bioxide of lead, have, of undergoing an explosive deoxidizement in consequence of mere superficial contact, is evidently another case, since the reaction is reciprocal. In the solution of the alloy of platina with silver, one body induces another to undergo the oxidizement to which it is itself subjected. In the case of the bioxide of hydrogen, and oxide of silver, two bodies, both prone to deoxidizement,



reciprocally induce that species of change. But in this phenomenon there is no third body to perform a part analogous to that of the nitric acid.

In case of ferments there is not only the power to produce *a* change, but also to produce *the* particular changes by which sugar, alcohol, and acetic or lactic acid, and mannite, are respectively generated. Moreover, these bodies are themselves undergoing an oxidation or decomposition which is necessary to their power; but this change is not like that which they induce. Hence, obviously, they operate differently, either from the platina sponge, or platina black, or from the silver in the alloy formed by it with platina. Liebig conceives, that this increased solubility of platina by union with silver, is at war with electro-chemical principles, agreeably to which, any metal in contact with another metal, relatively electro-positive, becomes less susceptible of attack. But this is not alleged of two metals in chemical combination, but of masses in contact, or having a metallic conductor extending from one to the other. I am surprised that Liebig should find the mystery of catalysis lessened by the solution of the alloy alluded to, when it must be evident that if the oxidation of one atom were a sufficient reason why another atom combined with it should be oxidized, an alloy of gold with silver ought to be soluble. Whereas, it is known that the common process of parting is founded on the utter insolubility of gold when so alloyed.

Liebig alleges that there can be no doubt that the acidification of alcohol is of the same order as the reaction by which nitric oxide provokes the formation of sulphuric acid in the leaden chamber, in which process the oxygen of the air is transferred to sulphurous acid by the intervention of the bioxide of nitrogen, since, in like manner organic substances associated with spirit of wine absorb oxygen, and bring it into a particular state which renders it liable to be absorbed.

But in the case thus cited, for every equivalent of acid formed, an equivalent of the bioxide combines first with an equivalent of oxygen, and in the next place with an equivalent of the sulphurous acid, forming a compound which is decomposed by water into sulphuric acid and the regenerated bioxide. There appears to me to be no analogy between this process and that of the influence of matter existing in no equivalent proportion, and which cannot be shown to form a definite chemical compound, either with acetyl or hydrogen. It is not represented that, in the vinous fermentation, any union, either transient or permanent, takes place between the elements of the sugar and those of the ferment; on the contrary it is alleged, that the oxidation and precipitation of the yeast proceeds *pari passu*, with the alcoholification.

As to all the processes referred to for illustration, as well as those of fermentation, which they are alleged to resemble, it appears to me that Liebig and his disciples have been too sanguine of their capacity to give adequate elucidation.

Respecting changes of the kind above described as *catalytic*, Kane uses the following language:—" *The elements of a compound are retained together in certain molecular arrangement, because the affinities are there satisfied; but it is natural to suppose that whilst the elements remain the same, their affinities for each other might be just as completely satisfied by a different molecular arrangement.*" This language might be held more reasonably, were this variation in arrangement accompanied by no concomitant acquisition of chemical properties; but is it reasonable to consider the difference between sugar, and the alcohol and carbonic acid into which it is resolvable, as arising merely from molecular arrangement? Can the active influence of alcohol upon the animal nerves be due merely to the situations respectively occupied by its three ultimate ponderable elements, carbon, hydrogen, and oxygen, of which it consists? Admitting that



the union of oxygen with the ingredients of gluten could, by imparting any consequent mechanical impulses, cause the hydrogen and oxygen of an atom of water to unite with the elements of sugar, and to separate into alcohol and carbonic acid as above mentioned, how can that movement, or the consequent rearrangement of the ponderable particles, explain the acquisition of new properties, of which the combining atoms, or the compounds previously containing them, were destitute? That the presence of yeast induces the fermentation of alcohol, and that diastase determines the generation of sugar, is admitted; but I am surprised that any philosopher should conceive, that without first ascertaining upon what the difference of the properties of alcohol and sugar is dependent, we can understand how that difference is caused. Liebig infers that a body in the act of decomposition, or combination, may communicate a movement to the atoms of an adjoining compound, so that gluten in the state of oxidation, in which it is called yeast, induces sugar,  $C_{12}H_{11}O_{11}$ , existing in the same liquid, to unite with the elements of water, making  $C_{12}H_{12}O_{12}$ , separating into four equivalents of carbonic acid and two of alcohol.

Adopting the same views as Liebig, Kane alleges, "that the slow decomposition of diastase communicates to the molecules of many thousand times its weight of starch, the degree of motion necessary for their rearrangement, and the appropriation of the elements of water requisite for the formation of starch sugar."

It is perfectly evident, that the particles of the catalyzed substance are in some way so affected by the catalyzing body as to be put into a state of reaction, which had not otherwise ensued; but that this is accomplished merely by imparted motion appears to me to be a surmise destitute of plausibility. The fact that the weight of the diastase requisite to saccharify starch is so very small, as is alleged by Kane, evidently renders

it extremely improbable that it acts by creating any mechanical disturbance. Yet this respectable chemist is so completely carried away by his idea, that he proceeds to make the following remark:

*"This law, of which the simplest expression is that where two chemical substances are in contact, any motion occurring among the particles of the one may be communicated to the other, is of a more purely mechanical nature than any other principle yet received in chemistry; and when more definitely established by succeeding researches, may be the basis of a dynamic theory in chemistry, as the law of equivalents and multiple combination expresses the statical condition of bodies which unite by chemical force."*

I perfectly agree in opinion with the author of these suggestions, as to the *purity of the mechanical attributes of the principle on which they are founded*, but cannot on this very account deem them competent to explain the phenomena on which he conceives them to bear.

As the mechanical influence of the motion of bodies is as the weight multiplied by the velocity, is it conceivable that any movement in the particles of one part, by weight, of diastase, can be productive of analogous movements in two thousand parts of starch?

The idea that yeast might owe its power to animalcules, suggested itself to me more than thirty years ago, and seems to have some support from the fact, that fermentation only thrives within the range of temperature compatible with animal life. Latterly, its activity has been ascribed to the power of extremely minute vegetables. Kane, while admitting the existence in yeast of a vast number of globular bodies, possibly animalcules, treats the idea as untenable, because the weight of the alcohol and carbonic acid is greater than that of the sugar employed. But if the union of water with the elements of the sugar, can add to the weight of the



products, without the assistance of animalcules, wherefore should their agency be inconsistent with an augmentation from the same source? But the weight of the alcohol and carbonic acid are just equal to that of the sugar, if this be assumed to be in the state of sugar of grapes.

Independently of any agency of this kind, which seems even more probable in the case of some species of infection, than in that of fermentation, I conceive that the present state of our knowledge does not allow of our comprehending the means by which bodies, whether organic or inorganic, are endowed with the powers ascribed to catalysis; but that we have great reason to believe that these powers, as well as all the properties which ultimate elements acquire by diversity of association, as in compound radicals, are due to the same source as the phenomena of galvanic and statical electricity.

It is well known, that although pure zinc is not susceptible of oxidation by exposure to dilute sulphuric acid, yet that, when containing minute proportions of other metals, as in the case of commercial zinc, it becomes liable to rapid oxidation by the same reagent. This Faraday explained by the electro-chemical influence of the comparatively electro-negative metallic particles distributed throughout the mass of the zinc, which he conceived to be productive of as many local galvanic circuits with corresponding currents. This explanation has, I believe, been universally sanctioned, and was consistent with the previous discovery of Sturgeon, that when, by amalgamating the surface with mercury, a metallic communication was made between the electro-positive and electro-negative metallic particles, so as to prevent the formation of electrolytic currents through the oxidizing liquid, the zinc became nearly as insusceptible of union with oxygen, as when in a pure state.

Nevertheless, either when pure, or when amalgamated, the zinc was found oxidizable by diluted sulphuric acid, provided it were made the element of a galvanic pair.

The facts above mentioned having been recalled to the attention of the scientific reader, I beg leave to inquire whether the influence thus ascribed by Faraday to the electro-negative metallic particles has not a greater analogy with that of a ferment, than those which have been brought forward by Liebig, Kane and others, with a view to explain the influence of that class of agents upon mechanical and chemical principles? Wherefore may not the distribution of nitrogenated substances throughout a mass of inorganic matter, operate as do the metallic impurities in commercial zinc? The existence of a powerful voltaic series in the gymnotus and other electrical fishes, shows that the substances which enter into the composition of animal matter are, when duly associated, as capable as metals of forming the elements not only of simple, but of complex galvanic circuits."

And thus the discussions proceed in ways indicating a master mind, zealous of bringing to his hearers, the truth relative to the objects of their study.

Indeed, the study of Hare's "Compendium" impresses one throughout with the wonderful originality of the man. At the time of the appearance of the first edition of this truly monumental work there were really no satisfactory textbooks in use from the hands of Americans. Those in vogue were in the main American editions of English works. So that in Hare's presentation of the science there is displayed the greatest originality. No other writer indulged so extensively in the discussion of constitution of bodies as did Hare. His ideas are strictly his own. The emphasis laid by him on physical phenomena is noteworthy. In modern times it would be said that Hare realized fully the importance which physics bore to chemistry. Were he active to-day he would undoubtedly be classed in the group of physical chemists.

One also wonders on reading the "Compendium" how a student body would view the necessity of becoming familiar



with such a mass of facts; especially would this be true of students of medicine, who are not prone to give any too much time to physics or chemistry.

In 1831 Silliman presented to the public his "Elements of Chemistry." On turning its pages there is on all sides evidence of Hare's influence. Indeed, most of the illustrations are from the "Compendium." The two friends, when together, must have talked freely upon their favorite subject and in the main they agreed. Nowhere in the text of Silliman are there, however, such frequent allusions to the speculative side of the subject as may be found in the "Compendium." Galvanism or electricity is given a great deal of space, but it seemingly is not so highly regarded as by Hare. This particular subject comprises many pages of the "Compendium."

A contemporary reviewer wrote:

"This work was written for the author's pupils, and is made the companion of his public lectures. It contains a luminous and comprehensive sketch of scientific chemistry, and one as full as was consistent with the limits which the author had prescribed to himself, after allowing sufficient room for a detailed account of many varieties of chemical apparatus and experiments, especially those which have been the result of his own invention and ingenuity."

The "Compendium" was truly a remarkable production. Students of the present would derive a wealth of suggestions from its pages. Was it not Wilhelm Ostwald who said he often devoted the moments of lunch hour to leafing the standard journals dealing with chemistry? And from them he obtained a fund of knowledge and suggestions for research, truly astonishing. So also may it be said of the "Compendium;" it is highly suggestive along many lines.

In addition to this splendid piece of literary work, Hare edited as early as 1819, with the assistance of Dr. Franklin

Bache, the first edition of *Ure's Dictionary of Chemistry*, concerning which the reviewers of the day said:

"It included all the recent discoveries, digested with great skill, and presented in a neat, concise, and perspicuous style."

And in the same year an American Edition of Henry's Chemistry, in two octavo volumes, appeared. The work is sparsely illustrated, so that one wonders whether for this reason, among others, Hare was not induced to print his "Minutes," and later the comprehensive "Compendium." There is no evidence whatever in the latter that its author was at all influenced by his publication of the work of Henry, which passed through at least two editions in this country.

In 1840, Hare also published "A Brief Exposition of the Science of Mechanical Electricity, or Electricity Proper, with Engravings and Descriptions of the Apparatus Employed." In this volume his originality, in devising experimental proofs of his statements, is forcefully illustrated. He also edited a pamphlet on the "History of Electricity," and another on "The Origin and Progress of Galvanism, or Voltaic Electricity." He further published an extended essay "On Electro-Magnetism," called by him "a new science."

The most important portions of these memoirs eventually found their place in the last edition of the "Compendium." In addition to all this he published one hundred and fifty or more articles in the pages of the *American Journal of Science*, to which it is only fair to add his numerous verbal communications made so regularly at stated meetings of the American Philosophical Society.

On November 7, 1843, Hare delivered a lecture introductory to a course on chemistry which his students requested that they be permitted to print. The request was granted. The opening paragraphs dealt chiefly with the wonders of science in general; indeed, he rambled about in a highly speculative region, finally abandoning it to consider to some extent



the science which he was accustomed to teach. It was then that he took occasion to advert to the wonderful character of the elements hydrogen, oxygen, carbon and nitrogen, saying that "the organic branch of chemistry has been so extended and modified, both as respects facts and hypotheses, that it now occupies as large a space in elementary treatises, as all the rest of the science, including inorganic chemistry, together with the auxiliary branches of statical, voltaic and magnetic electricity." After this he ventured to comment on some of "Liebig's physiological speculations." Granting that the latter had advanced "many ingenious ideas . . . highly serviceable to physiological chemistry" he had been altogether "bold, hasty, inconsiderate and inaccurate." He remarks: "I would liken him to a military leader, who, after marching through a country, with drums beating and colors flying, should have his trumpets loudly sounded, as if a complete conquest had been effected, while leaving behind him many fortresses, of which the knowledge had prevented more cautious and considerate leaders from having previously undertaken the same expedition. Nevertheless, by these means the philosopher of Giessen has excited a degree of attention, in the great mass of physicians and agriculturists, which had never been gained, had he neither deluded himself nor the readers of his essays with the prospect of an elucidation of the mysteries of animal and vegetable physiology, which it is beyond the present state of chemistry to afford. Moreover, the popularity which he has thus gained, may lead others to follow in the same path, who may rectify his errors and remedy some of his omissions without impairing what is really true in his doctrines.

There can be no better exemplification of the errors to which Liebig is addicted, than his adoption of the following maxim: "There are many ways to the highest pinnacle of a mountain, but those only can hope to reach it who keep the

summit constantly in view." It must be evident to every person of experience, in ascending mountains, that although it may be necessary to keep the bearing of the summit in mind, our eyes must be upon the path; and that, in most cases, the safest and easiest mode of access, causes the summit to be concealed for a time. A person who should implicitly follow Liebig's advice, would probably fall over some precipice, or tumble into some fissure which might escape notice while keeping the summit of the mountain constantly in view. Is not the fallacious rule of action, above quoted, a good figurative illustration of a theorist, who, keeping his mind too much upon some hypothetical acme, overlooks insurmountable objections which a close attention to facts would make evident? Has not Liebig exemplified his own course? . . .

Ordinary fires being supported by the union of atmospheric oxygen with the charcoal and the hydrogen of fuel, while the respiration of animals is attended by a union between atmospheric oxygen and the carbon of the blood, it has long been apparent that a large consumption of oxygen must be thus necessarily occurring. On the other hand, the observation that from the leaves of vegetables, exposed in water to the solar rays, a copious emission of oxygen ensued, and in fact that carbon was found largely to enter into their composition, led very naturally to the inference, that animals inspire oxygen, and give out carbon dioxide, while vegetables, respiring the gaseous compound formed with carbon by oxygen, called carbonic acid gas, emit oxygen, retaining the carbon.

But some contradictory observations had caused this view of the subject, to be represented as incorrect; and the question has always been undecided. Liebig has with great ability taken that side of this question,<sup>11</sup> to which I have always adhered. He considers that the carbon in vegetables

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<sup>11</sup> This side of the question has been experimentally supported by Professor Daubeny, of Oxford, England.



is due to the absorption of carbonic acid, and infers that it is thus that the enormous consumption of oxygen by fires, and animal respiration is compensated. He shows by calculation, that agreeably to analysis, there are three thousand millions of millions of pounds of carbon in the air, in the state of carbonic acid, and infers that the carbon in all the mineral coal known bears but a small proportion to that thus existing in the aeriform state.

It is known that inland plants yield by incineration, potash, the active matter of common soap ley. Plants on the borders of the ocean yield soda, an analogous substance. In various species of grain, certain salts are found to exist always in a certain ratio. Now, however minute are the proportions of these substances, Liebig correctly avers, as I believe, that their absence incapacitates a soil for the successful cultivation of the kind of plant requiring them.

This distinguished chemist concurs with the celebrated Davy in representing plants as taking up all soluble matters presented to their roots in a sufficiently diluted state, but appears to be peculiar in the opinion that it is only that portion of carbon which is in the state of gaseous carbonic acid which forms their food.

According to Davy, Berzelius, and others, vegetable matter, constituting humus or *geine*, yields certain acids which, being absorbed, are the means of nutrition. But both Davy and Liebig, the latter especially, consider that carbonic acid is imbibed by the vegetable foliage, the carbon being assimilated and the oxygen exhaled. Of course water is all important, and appears to be received through the leaves, as well as the roots.

Lignin, which constitutes the fibres of wood, hemp, flax and those of plants in general; also sugars, gum, starch and other analogous vegetable products, consist merely of water and carbon. Nitrogen exists in plants in comparatively small proportion; yet its presence appears to be of primary

importance, since it has a sort of ubiquity in the organs and juices. But although this element forms nearly four-fifths of the atmosphere, it seems to be generally conceded, and is by Liebig urged, that it is not directly obtained from that source by vegetation. According to this philosopher, a previous conversion into ammonia, by a union with hydrogen, is requisite; this alkali, and the carbonic acid with which it unites, when exposed to the atmosphere, being mainly the food of plants. But though nitrogen pervades vegetable organization, it abounds in a larger proportion in that of animals. Hence, it has been a question how animals, feeding on vegetables only, are supplied with a sufficiency of nitrogen. It naturally occurred, in the case of vegetables, that they might derive it from the atmosphere during respiration. But experimental investigation has shown that there is no absorption of nitrogen, during that process, tending to justify this inference.

Thus, in the supply of nitrogen to the vegetable and animal creation, nature, from considerations which are inscrutable to human reason, prefers an indirect and precarious source, to one which is superabundant and always at hand. Nor is this the only instance. Fishes, which swim in an element consisting of eight parts in nine of oxygen, are dependent for this principle on the contact of their gills with a minute portion of air absorbed from the atmosphere.

But Liebig alleges that, as a large portion of vegetable diet merely serves to yield the carbon required for respiration, there is, in the residue, a due proportion of nitrogen to form flesh and blood; since it has been shown by recent analyses that, in beans, wheat, and other grain, there are substances capable of isolation, which are identical in composition with the fibrous matter of the blood or fibrin, and with serum or white of egg, called albumen, also with milk curd or casein. Thus animals find ready formed in some



parts of vegetables, if not in all, the ingredients of their flesh and blood. But some of the most abundant articles of vegetable food, such as sugar, starch, gum, fat, oil, etc., being devoid of nitrogen, cannot alone contribute to the formation of flesh. They go, says Liebig, to support the fire in the lungs where thirteen ounces and a half of carbon are, on an average, daily consumed by a man; causing as much heat as would raise three hundred and seventy pounds of water to the temperature of the blood.

It is alleged by the same author that all the oxygen, thus combined with carbon, is in the first instance taken up by the protoxide of iron in the venous blood, which, being consequently changed in colour, causes the reddening of the blood ere it passes into the arteries. To this it has been objected that the quantity of iron in the blood is inadequate to take up a sufficiency of oxygen; and it appears to me that were the fact to be as suggested, the heat would be evolved in the lungs where the absorption of oxygen takes place, not in the capillaries where it is transferred to carbon.

Moreover, I am of opinion that, as protoxide of iron is of a more dingy red than arterial blood, it would be incompetent to colour this liquid, as alleged, unless assisted by some other agent, such, for instance, as sulpho-cyanhydric acid, which has been heretofore represented as participating in the result.

It would seem, on the whole, that Liebig has, in this respect, contributed more to enforce than to alter the opinions offered by me on this subject in the former editions of my text book. Yet I have always thought that a machinery so complicated as that employed in the process of respiration, could not have been devised merely for the generation of animal heat or the oxidizement of carbon in the tissues, as Liebig seems to believe. It has struck me that the necessity of atmospheric oxygen to fishes would hardly be ascribed

satisfactorily to the ponderable matter thus received through their gills, or on any heat which it may produce. I have suspected that there was some imponderable fluid, supplied to the nervous system by the process in question, to which the classes of animals, enjoying the benefits of it extensively, are indebted for the superiority which they obviously possess over animals which do not enjoy that advantage to a similar extent.

One of the greatest services rendered by the author, whose opinions are under consideration is, as I think, in directing attention to the different offices performed by two classes of vegetable products which may be distinguished as nitrogenized and as devoid of nitrogen. All the various species of sugar, starch, gum or mucilage, oil, fat, and gelatine, are represented as having a tendency rather to go to the support of the respiratory process, or to produce obesity; while the fibrin and albumen of flesh and blood are sustained by those portions of animal and vegetable food which contain nitrogen in nearly the same proportion as it exists in them. The greater vigour of a horse when fed on oats or maize, is in this way explained, by the greater proportion of matter contained in such grain, which is of a nature to compensate the wear of the muscles.

Highly worthy of consideration, also, are Liebig's suggestions respecting the services rendered by theine, a peculiarly highly nitrogenized principle, common both to tea and coffee. Liebig ingeniously shows that this principle requires only an addition of water and oxygen in order to convert it into taurine, an active principle of the bile. The extensive use of tea and coffee by civilized nations thus appears to have been the result of a sort of instinctive empirical research, leading to beneficial results, which physicians were heretofore unable to appreciate or explain. In fact, as food, coffee, and tea were heretofore considered as almost valueless; but now it appears that they serve to furnish nitrogen in a more concen-



trated form to those whose indolent habits might be incompatible with the consumption of sufficient quantity of ordinary nutriment to obtain a requisite quantity of that element.

There is nothing which seems more completely impenetrable to the human mind than the power of vitality. Probably in no instance is this power better exemplified than in the changes which, by means of the vital spark, take place in seeds and eggs. In the latter, especially, the principle of life seems to hold in check those chemical affinities which, so soon as it is extinct, convert into a putrid mass that which, life enduring, would be transformed into a young bird. The vital power of animal and vegetable organization, not only counteracts the conflicting affinities of inorganic atoms; it also endows groups, constituted of little else than three or four of those atoms, with powers analogous to those inherent in simple elementary atoms, and thus extends immeasurably the bounds of useful chemical reaction.

I infer that the organs of animals and vegetables have two modes of effecting the object for which they were created. In one mode, in which chemical reaction would fail to accomplish the requisite transformations, being such as affect masses rather than their component atoms, the organs react directly, in a mode entirely hidden from our view. There is, as Liebig justly alleges of such phenomena, an invisible cause. In the other mode, creating such chemical compounds as are suitable for their purposes, it may more or less leave to these the issue.

Liebig asserts that "we shall obtain that which is obtainable in a rational enquiry into nature, if we separate the actions belonging to chemical powers, from those which are subordinate to other influences;" but the learned author does not show us how we may accomplish this separation; and probably for the best possible reason that, great as are undoubtedly his skill and his genius, he is incompetent to effect any such separation. He seems to forget that he elsewhere

admits "chemical powers to be subordinate to other influences, whether of life, of heat, or electricity." To me it seems, that to separate the action of these powers from such as are subordinate to other influences, would involve their separation from themselves; and that it were inconsistent to suppose that chemical agents, which are created by the vital power, cannot be also modified by it so long as it prevails.

But, says this distinguished author: "the expression vital principle, must, meanwhile, be considered as equivalent to the terms 'specific' and 'dynamic' in medicine. *Everything is specific which we cannot explain, while, by the epithet dynamic, everything is explained which we do not understand.*"

This disparaging language, as respects the power of life, seems to me not quite consistent with the following opinions elsewhere stated by the celebrated author.

Thus the author, notwithstanding his contemptuous allusion to the vital principle, and his confounding it in value with words alleged by him to be insignificant, in the passage last quoted, justly ascribed to the principle in question a prodigious efficacy.

In the following passage Liebig's mode of reasoning is exemplified: "Is it truly vitality which generates sugar in the germ for the nutrition of young plants, or which gives to the stomach the power to dissolve and prepare for assimilation all the matter introduced into it? A decoction of malt possesses as little power to reproduce itself, as the stomach of a dead calf; both are unquestionably destitute of life, but when amylin or starch is introduced into a decoction of malt, it changes first into a gummy like matter, and lastly into sugar. Hard boiled albumen and muscular fibre can be dissolved in a decoction of a calf's stomach, to which a few drops of muriatic acid have been added precisely as in the stomach itself. The power, therefore, to effect transforma-



tions does not belong to the vital principle. Each transformation is *owing to a disturbance in the attraction of the elements of a compound, and is consequently a purely chemical process.*"

But is there any truth in the allegation that in no other than a chemical process, can there be any disturbance in the attraction of the elements of a compound? Is it by a chemical action that an electrical current subverts chemical affinities? Is it by a chemical action that vitality endows chemical compounds with peculiar attractive powers? Has not Liebig sanctioned the opposite idea in the passages which I have cited?

I conceive that plants and animals consist, in the first place, of organs and whatever may be necessary to the preservation or efficacy of their organs; secondly, of substances secreted or excreted by those organs; and, thirdly, of the compounds arising from the reaction of such substances with each other, or with extraneous chemical agents with or without an elevation of temperature. In an egg we have an organic mass possessing on the one hand the wonderful vital power to which allusion has been made, on the other containing albumen, a substance endowed with chemical affinities for certain oxides and chlorides. But the power which albumen has of contributing to the birth of the chicken, is quite distinct from that, which, after the vitality of the egg is destroyed, renders it an antidote for corrosive sublimate. Still more is the power of the yolk to constitute a living being, distinct from that by which, when ignited with potash and iron, it may give rise to two cyanides as in the well-known cyano-ferrite of potassium or ferro-prussiate of potash.

The germination of barley, by means of which it is malted, is so dependent upon the vital principle, that, when spontaneously heated by lying in large masses on shipboard or otherwise, it becomes incapable of the process above men-

tioned. Yet, by this vital process, a chemical change is induced in the organic mass, by which it is more or less transformed into a sweet soluble matter, called, when in solution, wort. This change is effected by the intermediate agency of diastase, a substance elaborated from malt. Thus, besides the greater mystery of life, we have the lesser mystery of the changes effected by the "action or presence of catalysis," as the process is designated by which diastase, or sulphuric acid, causes starch to be converted into grape sugar, and yeast converts sugar into alcohol and carbonic acid.

The saccharine matter produced by diastase, or otherwise, may, by nitric acid, be converted into oxalic acid. Thus we have four states in which new organic substances are produced. First, the vital organ, endowed with the germ of a living plant; secondly, an instrument produced by that organ, and possessing a sort of magic power of inducing chemical changes in substances with which it does not combine; thirdly, a chemical compound, elaborated by this chemical magic, and lastly, a product resulting from the reaction of the chemical compound with an inorganic reagent.

Among the greatest wonders of organic chemistry, is the acquisition of power, by elements otherwise inert, from mere grouping. The hempen cable, of which a given section has more strength than an equal weight of iron forming a chain, consists of nothing but water and carbon, into which it is easily resolved by the application of heat. By fire we may fuse or oxidise the iron, and thus equally deprive its particles of strength; but, on collecting the resulting fragments, metallurgic skill can elaborate another chain, the cohesive power of the metallic particles having been subdued, not destroyed; but no human skill, unaided by the powers of vegetable life, can regenerate another cable from water and carbon.

Subjected to ultimate analysis, prussic acid, which is so fatal to animals, consists only of three of the ingredients of



their flesh or blood. It is constituted of nitrogen, one of the elements of the air which animals breathe, of hydrogen, one of the elements of the water which they drink, and of charcoal which, per se, is inert.

By a proximate analysis, this deleterious acid is found to consist of hydrogen and cyanogen, a gaseous body, formed of one atom of nitrogen and two of carbon, being, therefore, a bicarburet of nitrogen.

Cyanogen is the first discovered of an important class of bodies now called compound radicals by the school of Liebig. These I would prefer to designate as *compound elements*, inasmuch as they are endowed with all the attributes of simple elements. Upon the idea thus exemplified, the existence in organic substances of various other compound radicals, has been inferred; not only when capable of isolation, like cyanogen, but also where they are known only in a state of combination. The discovery of the existence of these bodies forms a new era in our science.

Liebig designates organic chemistry as "*the chemistry of compound radicals.*" Nearly twenty *primary*, or derivative compound organic radicals, have been inferred to exist, of which nearly an equal number are severally generators of acids and bases.

By Liebig the diversity of these radicals, as respects properties, is to be ascribed either to the proportions by weight, in which the ultimate elements are present, or, when the proportions of these are equal, to the mode in which they are grouped. But I conceive that without resorting to the assistance of causes on which heat, light, electrical reaction, and nervous influence are dependent, the proportions or the groupings of their ponderable elements furnishes no adequate cause of the wonderful diversity, and astonishing activity of certain organic products.

Agreeably to Faraday's inferences, a grain of water or a

grain of zinc contains as much electricity as eight hundred thousand square feet of well charged coated glass surface. Admitting that these inferences are greatly beyond a true estimate, with such experimental evidence before us, is it reasonable to overlook the quality of matter on which its electrical efficacy is founded? Of the existence of some potent cause of electrical phenomena there can be no doubt; and whether it be of one nature or another, certainly it plays a part of infinite importance throughout the creation.

We may have under our eyes two little heaps of powder, conveying to the senses no proof that there is any difference in their composition, or any potency peculiar to either. One of these, subjected to the blow of a hammer, will explode with a startling report, and with violence sufficient to indent the steel; the other will cause, under like circumstances, no similar result; but if swallowed by an animal will be productive of death. A drop of prussic acid may produce an effect no less fatal by falling on the tongue of a dog, constituted of the same simple ponderable elements as the acid. The consequences are more like those of lightning, than such as would result from the impression made by a poison, agreeably to the idea of poisons generally entertained.

We have innumerable essential oils, spices, and other pungent vegetable productions, such as cinnamon, pepper, mustard, and horse-radish, which have upon the organ of taste, smell, or feeling, an endless variety of effects. Of the substances thus alluded to, many consist of nearly an equal number of atoms of carbon and hydrogen, while the rest vary only in having, in addition to these ingredients, a small proportion of oxygen. It is quite a mystery how, by the powers of vegetable life, these ponderable atoms are made to acquire such various qualities; but, as in the case of the fulminating powder, I have ascribed the result to the agency of imponderable matter, so in the case of the active substances engen-



dered by vitality, I should make a similar suggestion; their adventitious chemical, medicinal, or poisonous energy being due to the association of imponderable matter with groups of ponderable atoms.

It seems to me rather unreasonable in Liebig to speak so boldly, as he is wont, respecting physiological phenomena, while making no effort to explain the part performed by electricity in regard to them. Is there not reason to suppose that he has been so much occupied by the analytical department, that he is not sufficiently aware of the difficulty of doing justice to the electro-chemical department of physiology?

The power of producing all the phenomena of voltaic electricity, which the *gymnotus electricus* has been fully shown to possess, can leave no doubt respecting the capacity of the animal organization to generate electricity. It will be admitted that the animal nerves have functions to perform of the highest importance to animals; and of all known agents is there any which can be conceived to be the medium of their efficacy, excepting the electric fluid, or that cause of electrical phenomena which is usually thus designated.

So long as there is so much evidence of the potentiality of electro-chemical reactions, whatever may be their cause, and so long as we remain ignorant of the manner in which vital, electrical, and chemical forces are associated, is it not premature to expect any satisfactory explanation of the processes of life?"

Hare's intense love for everything in which his favorite subject—electricity—entered, prompted him, after reading Faraday's immortal *Researches*, to address (1840) this student of nature.

"Dear Sir,—

I have been indebted to your kindness for several pamphlets comprising your researches in electricity, which I have perused with the greatest degree of interest.

You must be too well aware of the height at which you stand, in the estimation of men of science, to doubt that I entertain with diffidence, any opinion in opposition to yours. I may say of you as in former instances of Berzelius, that you occupy an elevation inaccessible to unjustifiable criticism. Under these circumstances, I hope that I may, from you, experience the candor and kindness which were displayed by the great Swedish chemist in his reply to my strictures on his nomenclature.

I am unable to reconcile the language which you hold in paragraph 1615, with the fundamental position taken in 1155. Agreeably to the latter, you believe ordinary induction to be the action of *contiguous* particles, consisting of a species of polarity, instead of being an action of either particles or masses at "sensible distances." Agreeably to the former, you conceive that "assuming that a perfect vacuum was to intervene in the course of the line of inductive action, it does not follow from this theory that the line of particles on opposite sides of such a vacuum would not act upon each other." Again, supposing "it possible for a positively electrified particle to be in the centre of a vacuum an inch in diameter, nothing in my present view forbids that the particle should act at a distance of half an inch on all the particles forming the disk of the inner superficies of the bounding sphere."

Laying these quotations before you for reconsideration, I beg leave to inquire how a positively excited particle, situated as above described, can react "inductrically" with any particles in the superficies of the surrounding sphere, if this species of reaction require that the particles between which it takes place be contiguous. Moreover if induction be not "an action either of particles or masses at *sensible* distances," how can a particle situated as above described, "*act at the distance of half an inch on all the particles forming the disk of the inner superficies of the bounding sphere?*" What is a sensible distance, if half an inch is not?



How can the force thus exercised obey the "well known law of the squares of the distances," if as you state (1375) the rarefaction of the air does not alter the intensity of the inductive action? In proportion as the air is rarefied, do not its particles become more remote?

Can the ponderable particles of a gas be deemed contiguous in the true sense of this word, under any circumstances? And it may be well here to observe, that admitting induction to arise from an affection of intervening ponderable atoms, it is difficult to conceive that the intensity of this affection will be inversely as their number as alleged by you. No such law holds good in the communication of heat. The air in contact with a surface at a constant elevation of temperature, such for instance as might be supported by boiling water, would not become hotter by being rarefied, and consequently could not become more efficacious in the conduction of heat from the heated surface to a colder one in its vicinity.

As soon as I commenced the perusal of your researches on this subject, it occurred to me that the passage of electricity through a vacuum, or a highly rarefied medium, as demonstrated by various experiments, and especially those of Davy, was inconsistent with the idea that ponderable matter could be a necessary agent in the process of electrical induction. I therefore inferred that your efforts would be primarily directed to a re-examination of that question.

If induction, in action through a vacuum, be propagated in right lines, may not the curvilinear direction which it pursues, when passing through "dielectrics," be ascribed to the modifying influence which they exert?

If, as you concede, electrified particles on opposite sides of a vacuum can act upon each other, wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface, a contrary state, objectionable?

As the theory which you have proposed, gives great importance to the idea of polarity, I regret that you have not defined the meaning which you attach to this word. As you designate that to which you refer, as a "species of Polarity," it is presumable that you have conceived of several kinds with which ponderable atoms may be endowed. I find it difficult to conceive of any kind which may be capable of as many degrees of intensity as the known phenomena of electricity require; especially according to your opinion that the only difference between the fluid evolved by galvanic apparatus and that evolved by friction, is due to opposite extremes in quantity and intensity; the intensity of electrical excitement producible by the one, being almost infinitely greater than that which can be produced by the other. What state of the poles can constitute quantity—what other state intensity, the same matter being capable of either electricity, as is well known to be the fact? Would it not be well to consider how, consistently with any conceivable polarization, and without the assistance of some imponderable matter, any great difference of intensity in inductive power, can be created?

When by friction the surface is polarized so that particles are brought into a state of constraint from which they endeavour to return to their natural state, if nothing be super-added to them, it must be supposed that they have poles capable of existing in two different positions. In one of these positions, dissimilar poles coinciding, are neutralized; while in the other position, they are more remote, and consequently capable of acting upon other matter.

But I am unable to imagine any change which can admit of gradations of intensity, *increasing* with remoteness. I cannot figure to myself any reaction which increase of distance would not lessen. Much less can I conceive that such extremes of intensity can be thus created, as those of which you consider the existence as demonstrated. It may be sug-



gested that the change of polarity produced in particles by electrical inductions, may arise from the forced approximation of reciprocally repellent poles, so that the intensity of the inductive force, and of their effort to return to their previous situation, may be susceptible of the gradation which your electrical doctrines require. But could the existence of such a repellent force be consistent with the mutual cohesion which appears almost universally to be a property of ponderable particles? I am aware that, agreeably to the ingenious hypothesis of Mossotti, repulsion is an inherent property of the particles which we call ponderable; but then he assumes the existence of an imponderable fluid to account for cohesion; and for the necessity of such a fluid to account for induction it is my ultimate object to contend. I would suggest that it can hardly be expedient to ascribe the phenomena of electricity to the polarization of ponderable particles, unless it can be shown that if admitted, it would be competent to produce all the known varieties of electric excitement, whether as to its nature or energy.

If I comprehend your theory, the opposite electrical state induced on one side of a coated pane, when the other is directly electrified, arises from an affection of the intervening vitreous particles, by which a certain polar state caused on one side of the pane, induces an opposite state on the other side. Each vitreous particle having its poles severally in opposite states, they are arranged as magnetized iron filings in lines; so that alternately opposite poles are presented in such a manner that all of one kind are exposed at one surface, and all of the other kind at the other surface. Agreeably to this or any other imaginable view of the subject, I cannot avoid considering it inevitable that each particle must have at least two poles. It seems to me that the idea of polarity requires that there shall be in any body possessing it, two opposite poles. Hence you correctly allege that agreeably to your

views it is impossible to charge a portion of matter with one electric force without the other (see par. 1177). But if all this be true, how can there be a "positively excited particle?" (See par. 1616.) Must not every particle be excited negatively, if it be excited positively? Must it not have a negative, as well as a positive pole?

I cannot agree with you in the idea that consistently with the theory which ascribes the phenomena of electricity to one fluid, there can ever be an isolated existence either of the positive or negative state. Agreeably to this theory, any excited space, whether minus or plus, must have an adjoining space relatively in a different state. Between the phenomena of positive and negative excitement there will be no other distinction than that arising from the direction in which the fluid will endeavor to move. If the excited space be positive, it must strive to flow outward; if negative, it will strive to flow inward. When sufficiently intense, the direction will be shown by the greater length of the spark, when passing from a small ball to a large one. It is always longer when the small ball is positive, and the large one negative, than when their positions are reversed.

But for any current it is no less necessary that the pressure should be on one side comparatively minus, than that on the other side, it should be comparatively plus; and this state of the forces must exist whether the current originates from a hiatus before, or from pressure behind. One current cannot differ essentially from another, however they may be produced.

In paragraph 1330, I have been struck with the following query, "What then is to separate the principle of these extremes, perfect conduction and perfect insulation, from each other; since the moment we leave the smallest degree of perfection at either extremity, we involve the element of perfection at the opposite ends?" Might not this query be made with as much reason in the case of motion and rest,



between the extremes of which there is an infinity of gradations? If we are not to confound motion with rest, because in proportion as the former is retarded, it differs less from the latter; wherefore should we confound insulation with conduction, because in proportion as the one is less efficient, it becomes less remote from the other?

In any case of the intermixture of opposite qualities, may it not be said in the language which you employ "the moment we leave the element of perfection at one extremity, we involve the element of perfection at the opposite." Might it not be said of light and darkness, or of opaqueness and translucency; in which case to resort to your language again, it might be added "especially as we have not in nature, a case of perfection at one extremity or the other." But if there be not in nature, any two bodies of which one possesses the property of perfectly resisting the passage of electricity, while the other is endowed with the faculty of permitting its passage without any resistance; does this affect the propriety of considering the qualities of *insulation* and conduction in the abstract, as perfectly distinct, and inferring that so far as matter may be endowed with the one property, it must be wanting in the other?

Have you ever known electricity to pass through a pane of sound glass? My knowledge and experience create an impression that a coated pane is never discharged through the glass unless it be cracked or perforated. That the property by which glass resists the passage of electricity, can be confounded with that which enables a metallic wire to permit of its transfer, agreeably to Wheatstone's experiments, with a velocity greater than that of the solar rays, is to my mind inconceivable.

You infer that the residual charge of a battery arises from the partial penetration of the glass by the opposite excitements. But if glass be penetrable by electricity, why

does it not pass through it without a fracture or perforation?

According to your doctrine, induction consists "in a forced state of polarization in contiguous rows of the particles of the glass" (1300); and since this is propagated from one side to the other, it must of course exist equally at all depths. Yet the partial penetration suggested by you, supposes a collateral affection of the same kind, extending only to a limited depth. Is this consistent? Is it not more reasonable to suppose that the air in the vicinity of the coating gradually relinquishes to it a portion of free electricity, conveyed into it by what you call "convection." The coating being equally in contact with the air and glass, it appears to me more easy to conceive that the air might be penetrated by the excitement, than the glass.

In paragraph 1300, I observe the following statement: "When a Leyden jar is charged the particles of the glass are forced into this polarized and constrained condition by the electricity of the charging apparatus. Discharge is the return of the particles to their natural state, from their state of tension, whenever the two electric forces are allowed to be disposed of in some other direction." As you have not previously mentioned any particular direction in which the forces are exercised during the prevalence of this constrained condition, I am at a loss as to what meaning I am to attach to the words "some other direction." The word *some*, would lead to the idea that there was an uncertainty respecting the direction in which the forces might be disposed of; whereas it appears to me that the only direction in which they can operate, must be the opposite of that by which they have been induced.

The electrified particles can only "return to their natural state" by retracing the path by which they departed from it. I would suggest that for the words "*to be disposed of in some other direction*," it would be better to substitute the following, "to compensate each other by an adequate communication."



Agreeably to the explanation of the phenomenon of coated electrics afforded in the paragraph above quoted (1300), by what process can it be conceived that the opposite polarization of the surfaces can be neutralized by conduction through a metallic wire? If I understand your hypothesis correctly, the process by which the polarization of one of the vitreous surfaces in a pane produces an opposite polarization in the other, is precisely the same as that by which the electricity applied to one end of the wire extends itself to the other end.

I cannot conceive how two processes severally producing results so diametrically opposite as insulation and conduction, can be the same. By the former, a derangement of the electric equilibrium may be permanently sustained, while by the other, all derangement is counteracted with a rapidity almost infinite. But if the opposite charges are dependent upon a polarity induced in contiguous atoms of the glass, which endures so long as no communication ensues between the surfaces; by what conceivable process can a perfect conductor cause a discharge to take place, with a velocity at least as great as that of the solar light? Is it conceivable that all the lines of "contra-induction" or depolarization can concentrate themselves upon the wire from each surface so as to produce therein an intensity of polarization proportioned to the concentration; and that the opposite forces resulting from the polarization are thus reciprocally compensated? I must confess, such a concentration of such forces or states, is to me difficult to reconcile with the conception that it is at all to be ascribed to the action of rows of *contiguous ponderable particles*.

Does not your hypothesis require that the metallic particles, at opposite ends of the wire, shall in the first instance be subjected to the same polarization as the excited particles of the glass; and that the opposite polarizations, transmitted

to some intervening point, should thus be mutually destroyed, the one by the other? But if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated, when the discharge is sufficiently powerful? Their dissipation must take place either while they are in the state of being polarized, or in that of returning to their natural state. But if it happen when in the first mentioned state, the conductor must be destroyed before the opposite polarization upon the surfaces can be neutralized by its intervention. But if not dissipated in the act of being polarized, is it reasonable to suppose that the metallic particles can be sundered by returning to their *natural state* of depolarization?

Supposing that ordinary electrical induction could be satisfactorily ascribed to the reaction of ponderable particles, it cannot, it seems to me, be pretended that magnetic and electro-magnetic induction is referable to this species of reaction. It will be admitted that the Faradian currents do not for their production require intervening ponderable atoms.

From a note subjoined to page 37 of your pamphlet, it appears that "on the question of the existence of one or more imponderable fluids as the cause of electrical phenomena, it has not been your intention to decide." I should be much gratified if any of the strictures in which I have been so bold as to indulge, should contribute to influence your ultimate decision.

It appears to me that there has been an undue disposition to burden the matter, usually regarded as such, with more duties than it can perform. Although it is only with the properties of matter that we have a direct acquaintance, and the existence of matter rests upon a theoretic inference that since we perceive properties, there must be material particles to which those properties belong; yet there is no conviction which the mass of mankind entertain with more firmness than



that of the existence of matter in that ponderable form, in which it is instinctively recognized by people of common sense. Not perceiving that this conviction can only be supported as a theoretic deduction from our perception of the properties; there is a reluctance to admit the existence of other matter, which has not in its favor the same instinctive conception, although theoretically similar reasoning would apply. But if one kind of matter be admitted to exist because we perceive properties, the existence of which cannot be otherwise explained, are we not warranted, if we notice more properties than can reasonably be assigned to one kind of matter, to assume the existence of another kind of matter?

Independently of the considerations which have heretofore led some philosophers to suppose that we are surrounded by an ocean of electric matter, which by its redundancy or deficiency is capable of producing the phenomena of mechanical electricity, it has appeared to me inconceivable that the phenomena of galvanism and electro-magnetism, latterly brought into view, can be satisfactorily explained without supposing the agency of an intervening imponderable medium by whose subserviency the inductive influence of currents or magnets is propagated. If in that wonderful reciprocal reaction between masses and particles, to which I have alluded, the polarization of condensed or accumulated portions of intervening imponderable matter, can be brought in as a link to connect the otherwise imperfect chain of causes; it would appear to me a most important instrument in lifting the curtain which at present hides from our intellectual vision, this highly important mechanism of nature.

Having devised so many ingenious experiments tending to show that the received ideas of electrical inductions are inadequate to explain the phenomena without supposing a modifying influence in intervening ponderable matter, should there prove to be cases in which the results cannot be satis-

factorily explained by ascribing them to ponderable particles, I hope that you may be induced to review the whole ground, in order to determine whether the part to be assigned to contiguous ponderable particles, be not secondary to that performed by the imponderable principles by which they are surrounded.

But if galvanic phenomena be due to ponderable matter, evidently that matter must be in a state of combination. To what other cause than an intense affinity between it and the metallic particles with which it is associated, can its confinement be ascribed consistently with your estimate of the enormous quantity which exists in metals? If "a grain of water, or a grain of zinc, contain as much of the electric fluid as would supply eight hundred thousand charges of a battery containing a coated surface of fifteen hundred square inches," how intense must be the attraction by which this matter is confined? In such cases may not the material cause of electricity be considered as latent agreeably to the suggestion of Oersted, the founder of electro-magnetism. It is in combination with matter, and only capable of producing the appropriate effects of voltaic currents when in act of transfer from combination with one atom to another; this transfer being at once an effect and a cause of chemical decomposition, as you have demonstrated.

If polarization in any form, can be conceived to admit of the requisite gradations of intensity, which the phenomena seem to demand; would it not be more reasonable to suppose that it operates by means of an imponderable fluid existing throughout all space, however devoid of other matter? May not an electric current, so called, be a progressive polarization of rows of the electric particles, the polarity being produced at one end and destroyed at the other incessantly, as I understood you to suggest in the case of contiguous ponderable atoms.

When the electric particles within different wires are



polarized in the same tangential direction, the opposite poles being in proximity, there will be attraction. When the currents of polarization move oppositely, similar poles coinciding, there will be repulsion. The phenomena require that the magnetized or polarized particles should be arranged as tangents to the circumference, not as radii to the axis. Moreover, the progressive movement must be propagated in spiral lines in order to account for rotary influence.

Between a wire which is the mean of a galvanic discharge and another not making a part of a circuit, the electric matter which intervenes, may, by undergoing a polarization, become the medium of producing a progressive polarization in the second wire moving in a direction opposite to that in the inducing wire; or in other words an electrical current of the species called Faradian may be generated.

By progressive polarization in a wire, may not stationary polarization, or magnetism be created; and reciprocally by magnetic polarity may not progressive polarization be excited?

Might not the difficulty, above suggested, of the incompetency of any imaginable polarization to produce all the varieties of electrical excitement which facts require for explanation, be surmounted by supposing intensity to result from an accumulation of free electric polarized particles, and quantity from a still greater accumulation of such particles, polarized in a latent state or in chemical combination?

There are, it would seem, many indications in favor of the idea that electric excitement may be due to a forced polarity, but in endeavoring to define the state thus designated, or to explain by means of it the diversities of electrical charges, currents and effects, I have always felt the incompetency of any hypothesis which I could imagine. How are we to explain the insensibility of a gold leaf electroscope, to a galvanized wire, or the indifference of a magnetic needle to the most intensely electrified surfaces?

Possibly the Franklinian hypothesis may be combined with that above suggested, so that an electrical current may be constituted of an imponderable fluid in a state of polarization, the two electricities being the consequence of the position of the poles, or their presentation. Positive electricity may be the result of an accumulation of electric particles, presenting poles of one kind; negative, from a like accumulation of the same matter with a presentation of the opposite poles, inducing of course an opposite polarity. The condensation of the electric matter, within ponderable matter, may vary in obedience to a property analogous to that which determines the capacity for heat, and the different influence of dielectrics upon the process of electrical induction may arise from this source of variation.

With the highest esteem, I am yours truly,

ROBERT HARE."

Faraday's reply is so delightfully human and evidences the nobility of his great soul, that it is given here in part:

"My dear sir:

"London, England.

i. Your kind remarks have caused me very carefully to revise the general principles of the view of *static induction*, which I have ventured to put forth, with the very natural fear that as it did not obtain your acceptance it might be found in error; for it is not a mere complimentary expression, when I say, I have very great respect for your judgment. As the reconsideration of them has not made me aware that they differ amongst themselves or with facts, the resulting impression on my mind is that I must have expressed my meaning imperfectly; and I have a hope, that when more clearly stated, my views may gain your approbation. I feel that many of the words in the language of electrical science possess much meaning, and yet their interpretation by different philosophers often varies more or less, so that they do



not convey exactly the same idea to the minds of different men; this often renders it difficult when such words force themselves into use, to express with brevity as much as, and no more than, one really wishes to say.

ii. My theory of induction . . . makes no assertion as to the nature of electricity, . . . It does not even include the origination of the developed or excited state of the power or powers; but taking that as it is given by experiment and observation, it concerns itself only with the arrangement of the force in its communication to a distance in that particular yet very general phenomenon called static induction. . . .

iii. Bodies, whether conductors or non-conductors, can be charged. The word *charge* is equivocal; sometimes it means that state which a glass tube acquires when rubbed by silk, or which the prime conductor of a machine acquires when the latter is in action; at other times it means the state of a Leyden jar or similar inductive arrangement when it is said to be charged. . . .

vii. Simple charge therefore does not imply polarity in the body charged. Inductive charge . . . does (1672). The word charge, as applied to a Leyden jar or to the whole of any inductive arrangement, by including *all* the effects, comprehends of course both these states. . . .

xvi. In my papers I speak of all induction as being dependent on the action of *contiguous particles*; i.e. I assume that insulating bodies consist of particles which are conductors individually, but do not conduct to each other provided the intensity of action to which they are subject is beneath a given amount; and, that when the inductric body acts upon conductors at a distance, it does so by polarizing all those particles which occur in the portion of dielectric between it and them. I have used the term *contiguous*, but have, I hope, sufficiently expressed the meaning I attach to it: first by say-

ing "the next existing particle being considered as the contiguous one;" then in a note "I mean by contiguous particles those which are next to each other, not that there is no space between them," and, further, by the note to par. 1164 in the 8 vo. edition of my researches which is as follows: "The word contiguous is perhaps not the best that might have been used here and elsewhere, for as particles do not touch each other it is not strictly correct; I was induced to employ it because in its common acceptance it enabled me to state the theory plainly and with facility. By contiguous particles I mean those which are next.

xvii. Finally, my reasons for adopting the molecular theory of induction were, the phenomena of electrolytic discharge (1164, 1343); of induction in curved lines (1166, 1215); of specific inductive capacity (1167, 1252); of penetration and return action (1245); of difference of conduction and insulation (1320); of polar forces (1665), &c. &c.; but, for these reasons, and any strength and value they may possess, I refer to the papers themselves.

xviii. I will now turn to such parts of your critical remarks as may require attention. A man who advances what he thinks to be new truths, and to develop principles which profess to be more consistent with the laws of nature, than those already in the field, is liable to be charged, first with self-contradiction; then with the contradiction of facts; or he may be obscure in his expressions and so justly subject to certain queries; or he may be found in non-agreement with the opinions of others. The first and second points are very important, and every one subject to such charges, must be anxious to be made aware of, and also to set himself free from, or to acknowledge them. The third is also a fault to be removed if possible. The fourth is a matter of but small consequence in comparison with the other three; for as every man, who has the courage, not to say rashness, to form an



opinion of his own, thinks it better than any from which he differs, so it is only deeper investigation and, most generally, future investigators who can decide which is in the right.

xix. I am afraid I shall find it rather difficult to refer to your letter. I will however reckon the paragraphs in order from the top of each page, considering that the first which has its *beginning* first in the page. In referring to my own matter, I will employ the usual figures for the paragraphs of the experimental researches, and small Roman numerals for those of this communication.

xx. At par. 3, p. 1, you say you cannot reconcile my language at 1615 with that at 1165. In the latter place I have said, I believe ordinary induction in all cases to be an action of *contiguous* particles; and in the former, assuming a very hypothetical case, that of a vacuum, I have said nothing in my theory which forbids that a charged particle in the centre of a vacuum should act on the particle next to it, though that should be half an inch off. With the meaning which I have carefully attached to the word *contiguous*, xvi, I see no contradiction here in the terms used, nor any natural impossibility, or improbability in such an action. Nevertheless, all *ordinary* induction is to me an action of contiguous particles, being particles at insensible distances; induction across a vacuum is not an ordinary instance, and yet I do not perceive that it cannot come under the same principle of action.

xxi. As an illustration of my meaning, I may refer to the case parallel with mine, as to the extreme difference of interval between the acting particles or bodies, of the modern views of the radiation and conduction of heat. In radiation the rays leave the hot particles and pass occasionally through great distances to the next particle fitted to receive them; in conduction, where the heat passes from the hotter particles to those which are contiguous and form part of the same mass, still the passage is considered to be by a process precisely like

that of radiation; and though the effects are as is well known extremely different in their appearance, it cannot as yet be shewn that the principle of communication is not the same in both.

xxii. So on this point respecting contiguous particles and induction across half an inch of vacuum, I do not see that I am in contradiction with myself, or with any natural law or fact.

xxv. Par 3, page 2, is answered, except in the matter of opinion (xviii) according to my theory by xvi. The conduction of heat referred to in the paragraph itself, will, as it appears to me, bear no comparison with the phenomenon of electrical induction:—the first refers to the distant influence of an agent which travels by a very slow process, the second to one whose distant influence is simultaneous, so to speak, with the origin of the force at the place of action:—the first refers to an agent which is represented by the idea of one imponderable fluid, the second to an agency better represented probably by the idea of two fluids, or at least by two forces;—the first involves no polar action, nor any of its consequences; the second depends essentially on such action;—with the first, if a certain portion be originally employed in the centre of a spherical arrangement, but a small part appears ultimately at the surface; with the second, an amount of force appears instantly at the surface, (viii, ix, x, xi, xii, xiii, xiv,) exactly equal to the exciting or moving force which is still at the centre.

xxvi. Par. 2, page 4, involves another charge of self-contradiction, from which therefore I will next endeavor to set myself free. You say I “correctly allege that it is impossible to charge a portion of matter with one electric force without the other, (see par. 1177). But if all this be true how can there be a *positively excited particle*? (See par. 1616). Must not every particle be excited negatively if it be excited positively? Must it not have a negative as well



as a positive pole?" Now I have not said exactly what you attribute to me: my words are, "it is impossible experimentally to charge a portion of matter with one electric force *independently* of the other. Charge always implies *induction*, for it can in no instance be effected without." (1177). I can however easily perceive how my words have conveyed a very different meaning to your mind, and probably to others, than that I meant to express.

xxvii. Using the word charge in its simplest meaning, (iii, iv,) I think that a body *can* be charged with one electric force without the other, that body being considered in relation to itself only. But I think that such charge cannot exist without induction, (1178) or independently of what is called the development of an equal amount of the other electric force, not in itself, but in the neighboring consecutive particles of the surrounding dielectric, and through them of the facing particles of the uninsulated surrounding conducting bodies; which, under the circumstances terminate, as it were, the particular case of induction. I have no idea, therefore, that a particle when charged must itself, of necessity, be polar; . . .

xxviii. The third paragraph of page 6, includes the question, "is this consistent?" implying self-contradiction, which therefore I proceed to notice. The question arises out of the possibility of glass being a (slow) conductor or not of electricity; a point questioned also in the two preceding paragraphs. I believe that it is. I have charged small Leyden jars, made of thin flint glass tube, with electricity, taken out the charging wires, sealed them up hermetically, and after two or three years have opened and found no charge in them. . . .

xxx. The obscurity in my papers which has led to your remarks in par. 1, page 8, arises, as it appears to me (after my own imperfect expression,) from the uncertain or double

meaning of the word *discharge*. You say, "if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire; wherefore then are these dissipated when the discharge is sufficiently powerful?" A jar is said to be discharged when its charged state is reduced by any means, and it is found in its first indifferent condition; the word is then used simply to express the state of the apparatus, and so I have used it in the expressions criticised in par. 4 of page 6 already referred to. The *process* of discharge, or the mode by which the jar is brought into the discharged state, may be subdivided as of various kinds; and I have spoken of conductive (1320), electrolytic (1343), disruptive (1359), and convective (1562) discharge; . . . My view of the relation of insulators and conductors, as bodies of one class, is given at 1320, 1675, &c., of the researches; but I do not think the particles of the good conductors acquire an intensity of polarization anything like that of the particles of bad conductors. . . . The question of, why are the metallic particles dissipated when the charge is *sufficiently* powerful—is one that my theory is not called upon at present to answer; since it will be acknowledged by all that the dissipation is not necessary to discharge; that different effects ensue upon the subjection of bodies to different degrees of the same power is common enough in experimental philosophy; thus one degree of heat will merely make water hot whilst a higher will *dissipate* it as steam and a lower will convert it into ice.

xxxii. The next most important point, as it appears to me, is that contained in the third and fourth paragraphs of page 5. I have said (1330), "What, then, is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other; since the moment we leave in the smallest degree perfection at the opposite end?" and upon this you say, might not this query be made with



as much reason in the case of motion and rest?—and, in any case of the intermixture of opposite qualities may it not be said, the moment we leave the element of perfection at one end, we involve the element of perfection at the opposite? may it not be said of light and darkness, or of opaqueness and translucency? and so forth.

xxxiii. I admit that these questions are very properly put, not that I go to the full extent of them all, as for instance that of motion and rest, but I do not perceive their bearing upon the question of whether conduction and insulation are different properties dependent upon two different modes of action of the particles of the substances, respectively possessing these actions; or whether they are only differences in *degree* of one and the same mode of action? In this question, however, lies the whole gist of the matter. To explain my views, I will put a case or two. In former times a principle or force of levity was admitted as well as of gravity, and certain variations in the weights of bodies were supposed to be caused by different combinations of substances possessing these two principles. In later times the levity principle has been discarded; and though we still have imponderable substances, yet the phenomena concerning weight have been accounted for by one force or principle only, that of gravity; the difference in the gravitation of different bodies being considered due to differences in *degree* of this *one force* resident in them all. Now no one can for a moment suppose that it is the same thing, philosophically, to assume either the two forces or the one force, for the explanation of the phenomena in question.

xxxiv. Again;—at one time there was a distinction taken between the principle of heat and that of cold; at present that theory is done away with and the phenomena of heat and cold are referred to the same class (as I refer those of insulation and conduction to one class) and to the influence of

different degrees of the same power. But no one can say that the two theories, namely, that including but one positive principle and that including two, are alike.

xxxv. Again, there is the theory of one electric fluid and also that of two. One explains by the difference in degree or quantity of one fluid what the other attributes to the variation in the quantity and relation of two fluids. Both cannot be true; that they have nearly equal hold of our assent is only a proof of our ignorance; and it is certain, whichever is the false theory is at present holding the minds of its supporters in bondage and is greatly retarding the progress of science. . . .

xxxvii. I now come to what may be considered as queries in your letter, which I ought to answer. The second paragraph page 3 is one. As I concede that particles on opposite sides of a vacuum may perhaps act on each other, you ask "wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface a contrary state, objectionable?" My reasons for thinking the excited surface does not directly induce upon the opposite surface, &c., is first, my belief that the glass consists of particles, conductors in themselves but insulated as respects each other (xvii); and next that in the arrangement given iv, ix or x, A does not induce directly on C but through the intermediate masses or particles of conducting matter.

xxxviii. In the next paragraph the question is rather implied than asked, what do I mean by polarity? I had hoped that the paragraphs 1669, 1670, 1671, 1672, 1679, 1686, 1687, 1688, 1699, 1700, 1701, 1702, 1703, 1704, in the researches would have been sufficient to convey my meaning, and I am inclined to think you had not perhaps seen them when your letter was written. They, and the observations already made (v, xxvi), with the case given (iv, v), will I think be sufficient as my answer. The sense of the word *polarity* is so



diverse when applied to light, to a crystal, to a magnet, to the voltaic battery, and so different in all these cases to that of the word when applied to the state of a conductor under induction (v), that I thought it safer to use the phrase "species of polarity" than any other which, being more expressive, would pledge me farther than I wished to go.

xxxix. The next or fourth par. of page 3, involves a mistake of my views. I do not consider bodies which are charged by friction or otherwise as polarized, or as having their particles polarized (iii, iv, xxvii). This paragraph and the next do not require therefore any further remark, especially after what I have said of polarity above (xxxviii).

xl. And now, my dear sir, I think I ought to draw my reply to an end. The paragraphs which remain unanswered, refer, I think, only to differences of opinion, or else not even to differences, but opinions regarding which I have not ventured to judge. These opinions I esteem as of the utmost importance; but that is a reason which makes me the rather desirous to decline entering upon their consideration; inasmuch as upon many of their connected points I have formed no decided notion, but am constrained by ignorance and the contrast of facts, to hold my judgment as yet in suspense. It is indeed to me an annoying matter to find how many subjects there are in electrical science, on which if I were asked for an opinion, I should have to say I cannot tell—I do not know; but, on the other hand, it is encouraging to think that these are they which if pursued industriously, experimentally, and thoughtfully, will lead to new discoveries. Such a subject, for instance, occurs in the currents produced by dynamic induction, which you say it will be admitted do not require for their production intervening ponderable atoms. For my own part, I more than half incline to think they do require these intervening particles, i.e. when any particles intervene, (1729, 1733, 1735.) But on this ques-

tion, as on many others, I have not yet made up my mind. Allow me therefore here to conclude my letter, and believe me to be, with the highest esteem and respect, my dear sir, your obliged and faithful servant,

Royal Institution,

M. FARADAY."

April 18, 1840."

In a second letter (1845) Hare wrote as follows to Faraday:

"My dear Sir—

In the month of July last I had the pleasure to read, in the *American Journal of Science*, your letter in reply to one which I had addressed to you through the same channel. I should sooner have noticed this letter, but that meanwhile I have had to republish two of my text-books, and, besides, could not command, until lately, a complete copy of all those numbers of your researches to which you have referred.

The tenor of the language with which your letter commences realizes the hope, which I cherished, that my strictures would call forth an amicable reply. Under these circumstances it would grieve me that you should consider any part of my language as charging you with inconsistency or self-contradiction, as if it could be my object to put you in the wrong, farther than might be necessary to establish my conception of the truth. Certainly it has been my wish never to go beyond the sentiment, "*Amicus Plato, sed magis amica veritas.*" I attach high importance to the facts established by your "*Researches*," which can only be appreciated sufficiently by those who have experienced the labor, corporeal and mental, which experimental investigations require. I am moreover grateful for the disposition to do me justice, manifested in those researches; yet it may not always be possible for me to display the deference, which I nevertheless entertain. I am aware that when in a discussion, which due



attention to brevity must render unceremonious, diversities of opinion are exhibited, much magnanimity is requisite in the party whose opinions are assailed; but I trust that both of us have truth in view above all other objects; and that so much of your new doctrine as tends to promote that end, will not be invalidated by a criticism which, though free, is intended to be perfectly fair and friendly.

In paragraph (11) your language is as follows, "*my theory of induction makes no assertion as to the nature of electricity, nor at all questions any of the theories respecting that subject.*" Owing to this avowed omission to state your opinions of the nature of electricity as preliminary to the statement of your "*theory,*" and because I was unable to reconcile that theory with those previously accredited, I received the impression that you claimed no aid from any imponderable principle. It appeared to me that there was no room for the agency of any such principle, if induction were an *action* of contiguous ponderable particles, *consisting* of a species of polarity. It seemed to follow, that what we call electricity, could be nothing more than a polarity, in the ponderable particles, directly caused by those mechanical or chemical frictions, movements, or reactions by which ponderable bodies are electrified. You have correctly inferred that I had not seen the fourteenth series of your researches, containing certain paragraphs. From them it appears that the polarity, on which so much stress has been laid, is analogous to that which has long been known to arise in a mass, about which the electric equilibrium has been subverted, by the inductive influence of the electricity accumulated upon another mass. This is clearly explained in paragraph iv of your letter, by the illustration, agreeably to which three bodies, a, b, c, are situated in a line, in the order in which they are named, in proximity, but not in contact. "A is electrified positively and then C is uninsulated." It is evident that you are correct

in representing that under these circumstances the extremities of B will be oppositely excited, so as to have a reaction with any similarly excited body, analogous to that which takes place between magnets; since the similarly excited extremities of two such bodies, would repel each other; while those dissimilarly excited, would be reciprocally attractive. Hence no doubt the word polarity is conceived by you to convey an idea of the state of the body B. If I may be allowed to propose an epithet to convey the idea which I have of the state of mass thus electrified, I would designate it as an electropolar state, or as a state of electropolarity.

It does not appear to me that in the suggestion of the electropolarity which we both agree to be induced upon the body B (iv), so long as it concerns a mass, there is any novelty. The only part of your doctrine which is new, is that which suggests an analogous state to be caused in the particles of the bodies through which the inductive power is propagated. Admitting each of the particles of a dielectric, through which the process of ordinary induction takes place, to be put into the state of the body B, it does not appear to me to justify your own exemplification of that process, you should have alleged ordinary induction to be *productive* of an *affection* of particles *causing* in them a species of polarity. In the case of the bodies, A, B, C, (iv) B is evidently passive. How then can we consider as active, particles represented to be in an analogous state? If in B there is no action, how can there be any action in particles performing a perfectly similar part? Moreover, how can the inductive power of an electrical accumulation upon A, *consist* of the polarity which it induces in B?

Having supposed (viii,) an electrified ball, A, an inch in diameter, to be situated within a thin metallic sphere, C, of a foot in diameter, you suggest that were one thousand concentric metallic spheres interposed between A, and the inner



surface of C, the electro-polar state of each particle in those spheres would be analogous to that of B already mentioned. Of course if there be an action of those particles, there must be an action of B; but this appears to me not only irreconcilable with any previously existing theory, but also with your own exposition of the process by which B is polarized.

Supposing concentric metallic hemispheres were interposed only upon one side of A, you aver that agreeably to your experience, more of the inductive influence would be extended towards that side of the containing shell than before (xiv.) Admitting this, I cannot concede that the greater influence of the induction, resulting from the presence of the metallic particles, is the consequence of any *action* of theirs; whether in *contiguity* or in *proximity*. Agreeably to my view, the action is confined to the electrical accumulation in the sphere A. Between the electricity accumulated in this sphere, and that existing in, or about, the intervening ponderable particles, there may be a reaction; but evidently these particles are as inactive as are the steps of a ladder in the scaling of a wall.

Suppose a powerful magnet to be so curved as to have the terminating polar surfaces parallel, and leaving between them an interval of some inches. Place between these surfaces, a number of short pieces of soft iron wire. These would of course be magnetized, and would arrange themselves in rows, the north and south poles becoming contiguous. Would this be a sufficient reason for saying that the inductive influence of the magnetic poles was an *action* of the contiguous wires? Would not the phenomena be the consequence of an affection of the contiguous pieces of wire, not of their action?

As respects the word charge, I am not aware that I have been in the habit of attaching any erroneous meaning to it, as your efforts to define it in paragraph iii would imply. I



MEDALLION PORTRAIT  
By H. Saunders, 1856, Philadelphia





have been accustomed to restrict the use of it to the case which you distinguish as an inductive charge, illustrated by that of the Leyden jar. To designate the states of the conductors of a machine, I have almost always employed the words *excited* or *excitement*. In my text-book, these words are used to designate the state of glass or resin electrified by friction, while that of coated surfaces, whether panes or jars, inductively electrified, has been designated by the words *charge* or *charged*.

I understood the word contiguous to imply contact, or contiguity, whereas it seems that it was intended by you to convey the idea of proximity. In the last mentioned sense it is not inconsistent with the idea of an action at the distance of half an inch: but by admitting the word contiguous to be ill chosen, you have, with great candor, furnished me with an apology for having mistaken your meaning.

Any inductive action which does not exist at sensible distances, (xx) you attribute to *ordinary* induction, considering the case of induction through a vacuum as an *extraordinary* case of induction. To me it appears that the induction must be the same in both cases, and that the *circumstances* under which it acts, are those which may be considered in the one case as *ordinary*, in the other *extraordinary*. Thus, take the case cited in your reply (viii, ix, x). Does the interposition of the spheres alter the character of the inductive power in the sphere A?

Either the force exercised by the charge in A, is like that of gravitation, altogether independent of the influence of intervening bodies; or, like that of light, it is dependent on the agency of an intervening matter. Agreeably to one doctrine, the matter by means of which luminous bodies act, operates by its transmission from the luminous surface to that illumined. Agreeably to another doctrine, the illuminating matter operates by its transmission from the luminous sur-



face to that illumined. Agreeably to another doctrine, the illuminating matter operates by its undulations. If the inductive power of electrified bodies be not analogous to gravitation, it must be analogous to the power by which light is produced so far as to be dependent on intervening matter. But were it to resemble gravitation, like that force it would be uninfluenced by such matter. If your experiments prove that electrical induction is liable to be modified by intervening matter, it is demonstrated that in its mode of operation it is analogous to light, not to gravitation. It is then proved, that, agreeably to your doctrine, electrical induction requires the intervention of matter, but you admit that it acts across a vacuum, and of course, acts without the presence of *ponderable* matter. Yet it requires intervening matter of some kind, and, since that matter is not ponderable, it must of necessity be imponderable. When light is communicated from a luminous body in the centre of an exhausted sphere, agreeably to the undulatory hypothesis, its efficacy is dependent on the waves excited in an intervening imponderable medium. Agreeably to your electropolar hypothesis, the inductive efficacy of an electrified body in an exhausted sphere would be due to a derangement of electric equilibrium, by which an electric state opposite to that at the centre would be produced at the surface of the containing sphere (xxvi, xxvii). This case you consider as one of extraordinary induction, but when air is admitted into a hollow sphere, or when concentric spheres are interposed, you hold it to be a case of ordinary induction. Let us then, in the case of the luminous body, imagine that concentric spheres of glass are interposed, of which the surfaces are roughened by grinding. In consequence of the roughness thus produced, the rays instead of proceeding in radii from the central ball would be so refracted as to cross each other. Of the two instances of illumination, thus imagined, would the one be described as *ordinary*,

the other as *extraordinary radiation*? But if these epithets are not to be applied to radiation, wherefore under analogous circumstances are they applicable to induction? Wherefore is induction when acting through a plenum to be called ordinary, and yet when acting through a vacuum to be called extraordinary? In the well known case of the refracting power of Iceland spar, light undergoes an *ordinary* and *extraordinary refraction*; not an *ordinary* and *extraordinary radiation*. The candle, of which, when viewed through the spar, two images are seen, does not *radiate ordinarily* and *extraordinarily*.

If there be occasionally, as you allege (xxi,) large intervals between the particles of radiant heat, how can the distances between them resemble those existing between particles acting at distances which are not sensible? The repulsive reaction between the particles of radiant caloric, as described by you (xxi), resembles that which I have supposed to exist between those of electricity; but I cannot conceive of any description less suitable for either, than that of particles which do not act at sensible distances.

Aware that the materiality of heat, and the Newtonian theory, which ascribes radiation to the projection of heat or light producing particles, have been questioned, I should not have appealed to a doctrine which assumes both the materiality of heat, and the truth of the Newtonian theory, had not you led the way; but, agreeably to the doctrine and theory alluded to, I cannot accord with you in perceiving any similitude between the processes of conduction and radiation.

Consistently with the hypothesis that electricity is material, you have shewn that an enormous quantity of it must exist in metals. To me it seems equally evident that, agreeably to the idea that heat is material, there must exist in metals a proportionably great quantity of caloric. The intense heat produced when wires are deflagrated by an elec-



trical discharge, cannot otherwise be consistently accounted for. Agreeably to the same idea, every metallic particle in any metallic mass, must be surrounded by an atmosphere of caloric; since the *changes* of dimension consequent to variations of temperature, can only be explained by corresponding variations in the quantity of caloric imbibed, and in the consequent density of the calorific atmospheres existing in the mass which undergoes these changes.

Such being the constitution of expansible bodies, agreeably to the hypothesis in question, it seems to me that the process, by which caloric is propagated through them by *conduction*, must be extremely different from that by which it is transmitted from one part of space to another by *radiation*. In the one case the calorific particle flies, like a cannon ball, with an inconceivably greater velocity, which is not sensibly retarded by the reflecting or refracting influence of intervening transparent media: in the other case it must be slowly imparted from one calorific atmosphere to another, until the repulsion sustained on all sides is *in equilibrio*. It is in this way that I have always explained the fact that metals are bad radiators, while good reflectors.

In paragraph (xxv,) you allege that conduction of heat differs from electrical induction, because it passes by a very slow process; while induction is in its distant influence simultaneous with its force at the place of action. How then can the passage of heat by conduction, be “a process precisely like that of radiation,” (xxi,) which resembles induction in the velocity with which its influence reaches objects, however remote?

Although (xxi) you appeal to the “modern views respecting radiation and conduction of heat,” in order to illustrate your conception of the contiguity of the particles of bodies subjected to induction, yet in (xxv,) you object to the reference which I had made to these views, in order to

shew that the intensity of electropolarization could not be inversely as the number of particles interposed between the "inductric" surfaces. Let us then resort to that above suggested, of the influence of the poles of a magnet upon intervening pieces of iron wires. In 1679, 14th series, you suggest this as an analogous case to that of the process of *ordinary* electrical induction, which we have under consideration. Should there be in the one case a thousand pieces of wire interposed, in the second a hundred, will it be pretended that the intensity of their reciprocal inductive reaction would be inversely as the number; so that the effect of the last mentioned number of wires would be equivalent to that of the first? Were intervals to be created between the wires by removing, from among the number first mentioned, alternate wires, it would seem to me that the diminution of effect would be commensurate not only with the reduction of the number of the wires, but likewise with the consequent enlargement of the intervals.

If as you suggest, the interposition of ponderable particles have any tendency to promote inductive influence, (xiv,) there must be some number of such particles by which this effect will be best attained. That number being interposed, I cannot imagine how the intensity of any electropolarity, thus created in the intervening particles, can, by a diminution of their number, acquire a proportional increase; evidently in no case can the excitement in the particles exceed that of the "inductric" surfaces whence the derangement of electrical equilibrium arises.

The repulsive power of electricity being admitted to be inversely as the squares of the distances, you correctly infer that the aggregate influence of an electrified ball, B, situated at the centre of a hollow sphere, C, will be a constant quantity, whatever may be the diameter of C. This is perfectly analogous to the illuminating influence of a luminous



body situated at the centre of a hollow sphere, which would of course receive the whole of the light emitted whatever might be its diameter, provided that there were nothing interposed to intercept any portion of the rays. But in order to answer the objection which I have advanced, that the diminution of the density of a "dielectric" cannot be compensated by any consequent increase of inductive intensity, it must be shown in the case of several similar hollow spheres, in which various numbers of electrified equidistant balls should exist, that the influence of such balls upon each other, and upon the surfaces of the spheres, would not be directly as the number of the balls, and inversely as the size of the containing spaces. Were gas lights substituted for the balls, it must be evident that the intensity of the light, in any one of the spheres, would be as the number of lights which it might contain. Now one of your illustrations (viii,) above noticed makes light and electrical induction, obey the same law as respects the influence of distance upon the respective intensities.

From these considerations, and others above stated, I infer, that if electrical induction were an action of particles in proximity operating reciprocally with forces varying in intensity with the squares of the distances, their aggregate influence upon any surfaces, between which they might be situated, would be proportionable to their number; and since experience demonstrates that the inductive power is not diminished by the reduction of the number of the intervening particles I conclude that it is independent of any energy of theirs, and proceeds altogether from that electrical accumulation with which the inductive change is admitted to originate.

In paragraph (xxxi,) you say "that at one time there was a distinction between heat and cold. At present that theory is done away with, and the phenomena of heat and cold are referred to the same class, and to different degrees of the same power."

In reply to this I beg leave to point out, that although, in ordinary acceptation, cold refers to relatively low temperature; yet we all understand that there might be that perfect negation of heat, or abstraction of caloric, which may be defined absolute cold. I presume that, having thus defined absolute cold, you would not represent it as identical with caloric. For my own part this would seem as unreasonable as to confound matter with nihility.

Assuming that there is only one electric fluid, there appears to me to be an analogy between caloric and electricity, so far that negative electricity conveys, in the one case, an idea analogous to that which cold conveys in the other. But if the doctrine of Du Fay be admitted, there are two kinds of electric matter, which are no more to be confounded than an acid and an alkali. Let us, upon these premises, subject to further examination your argument (1330,) that insulation and conduction should be identified, "*since the moment we leave in the smallest degree perfection at either extremity, we involve the element of perfection at the opposite end.*" Let us suppose two remote portions of space, one, replete with pure vitreous electricity, the other with pure resinous: let there be a series of like spaces containing the resinous and vitreous electricities in as many different varieties of admixture, so that in passing from one of the first mentioned spaces, through the series to the other, as soon as we should cease to be exposed to the vitreous fluid, in perfect purity, we should begin to be exposed minutely to the resinous; or that, in passing from the purely resinous atmosphere, we should begin to be exposed to a minute portion of the vitreous fluid; would this be a sufficient reason for confounding the two fluids, and treating the phenomena to which they give rise as the effect of one only?

But the discussion, into which your illustrations have led me, refers to things, whereas conductors and insulation,



as I understand them, are opposite and incompatible properties, so that, in as much as either prevails, the other must be counteracted. Conduction conveys to my mind the idea of *permeability* to the electric fluid, insulation that of *impermeability*; and I am unable to understand how these irreconcilable properties can be produced by a difference of degree in any one property of electrics and conductors.

If, as you infer, glass has, comparatively with metals, an almost infinitely minute degree of the conducting power, is it this power which enables it to prevent conduction, or in other words to insulate? Let it be granted that you have correctly supposed conduction to comprise both induction and discharge, the one following the other in perfect conductors within an inexpressibly brief interval. Insulation does not prevent induction; but, so far as it goes, it prevents discharge. In practice this part of the process of conduction does not take place through glass during any time ordinarily allotted to our experiments, however correct you may have been in supposing it to have ensued before the expiration of a year or more in the case of the tubes which you had sealed after charging them. But conceding it to have been thus proved that glass had, comparatively with metals, an infinitely small degree of the conducting power; is it this minute degree of conducting power, which enables it to prevent conduction, or in other words to insulate?

Induction arises from one or more properties of electricity, insulation from a property of ponderable matter; and although there be no matter capable of preventing induction, as well as discharge, were there such a matter, would that annihilate insulation? On the contrary would it not exhibit the property in the highest perfection?

As respects the residual charge of a battery, is it not evident that any electrical charge which affects the surface of the glass, must produce a corresponding effect upon the

stratum of air in contact with the coating of the glass? If we place one coating between two panes, will it not enable us to a certain extent to charge or discharge both? Substituting the air for one of them, will it not, in some measure, be liable to an affection similar to that of the vitreous surface for which it is substituted? In the well known process of the condensing electrometer, the plate of air interposed between the disks is, I believe, universally admitted to perform the part of an electric, and to be equivalent in its properties to the glass in a coated pane.

When I adverted to a gradual relinquishment of electricity by the air to the glass, I did not mean to suggest that it was attended by any more delay than the case actually demonstrates. It might be slow or gradual, compared with the velocity of an electric discharge, and yet be extremely quick, comparatively with any velocity ever produced in ponderable matter. That the return should be slow when no coating was employed, and yet quick when it was employed, as stated by you (xxxviii,) is precisely what I should have expected; because the coating only operates to remove all obstruction to the electric equilibrium. The quantity or intensity of the excitement is dependent altogether upon the electrified surfaces of the air and the glass. You have cited (1632,) the property of a charged Leyden jar, as usually accoutered, of electrifying a carrier ball. This I think sanctions the existence of a power to electrify by "convextion," the surrounding air to a greater or less depth; since it must be evident that every aerial particle must be competent to perform the part of the carrier ball.

Agreeably to the Franklinian doctrine, the electricity directly accumulated upon one side of a pane repels that upon the other side. You admit that this would take place were a vacuum to intervene; but when ponderable matter is interposed, you conceive each particle to act as does the body



B when situated as described between A and C (iv.) But agreeably to the view which I have taken, and what I understand to be your own exposition of the case, B is altogether passive, so that it cannot help, if it does not impede the repulsive influence. Moreover, it must be quite evident, that were B removed, and A approximated to C, without attaining the striking distance, the effect upon C and the consequent energy of any discharge upon it from A would be greater instead of less. If in the charge of a coated pane the intermediate ponderable vitreous particles have any tendency to enhance the charge, how happens it that, the power of the machine employed being the same, the intensity of the charge which can be given to an electric is greater in proportion to its tenuity?

In reference to the direction of any discharge, it appears to me that, as in *charging*, the fluid must always pass from the cathode to the anode, so in reversing the process it must pursue, as I have alleged, the opposite course of going from the anode back to the cathode. Evidently the circumvolutions of the circuit are as unimportant as respects a correct idea of the direction, as their length has been shown by Wheatstone, to be incompetent to produce any perceptible delay.

The dissipation of conductors being one of the most prominent among electrical phenomena, it appears to me to be an objection to your theory, if while it fails to suggest any process by which this phenomenon is produced, it assumes premises which seem to be incompatible with the generation of any explosive power. If discharge only involves the restoration of polarized ponderable particles to their natural state, the potency of the discharge must be proportionable to the intensity of the antecedent polarity; yet it is through conductors, liable, as you allege, to polarization of comparatively low intensity (xxx), that discharge takes place with the highest degree of explosive violence.

Having inquired how your allegation could be true, that discharge brings bodies to their natural state and yet causes conductors to be dissipated, you reply (xxxiv) that different effects may result from the same cause acting with different degrees of intensity; as when by one degree of heat ice is converted into water, by another into steam. But it may be urged, that although in the case thus cited, different effects are produced, yet that the one is not inconsistent with the other, as were those ascribed to electrical discharges. It is quite consistent, that the protoxide of hydrogen which *per se* constitutes the solid called ice, should by one degree of calorific repulsion have the cohesion of its particles so counteracted as to be productive of fusion; and yet that a higher degree of the same power should impart to them the repulsive quality requisite to the aeriform state. In order to found upon the influence of variations of temperature, a good objection to my argument, it should be shown, that while a certain reduction of temperature enables aqueous particles to indulge their innate propensity to consolidation, a still further reduction will cause them, in direct opposition to that propensity, to repel each other so as to form steam.

In your concluding paragraph you allege, "that when ponderable particles intervene, during the process of dynamic induction, the currents resulting from this source do require these particles." I presume this allegation is to be explained by the conjecture made by you (1729) that since certain bodies when interposed did not interfere with dynamic induction, therefore they might be inferred to co-operate in the transmission of that species of inductive influence. But if the induction takes place without the ponderable matter, is it right to assume that this matter *aids* because it does not prevent the effect? Might it not be as reasonably inferred in the case of light, that although its transmission does not require the interposition of a pane of glass, yet that when



such a pane is interposed, since the light is not intercepted, there is reason to suppose an active co-operation of the vitreous particles in aid of the radiation? It may be expedient here to advert to the fact that Prof. Henry has found a metallic plate to interfere with the dynamic induction of one flat helix upon another. I have myself been witness of this result.

Does not magnetic or electrodynamic induction take place as well in vacuo as in pleno? Has the presence of any gas been found to promote or retard that species of reaction? It appears, that agreeably to your experiments, ponderable bodies, when made to intervene, did not enhance the influence in question; while in some of those performed by Henry it was intercepted by them. Does it not follow that ponderable particles may impede, but cannot assist in this process?

I am happy to find, that among the opinions which I expressed in my letter to you, although there are several in which you do not concur, there are some which you esteem of importance, though you do not consider yourself justified in extending to them your sanction; being constrained, in the present state of human knowledge, to hold your judgment in suspense. For the present, I shall here take leave of this subject, having already so extended my letter as to occupy too much of your valuable time. I am aware that as yet I have not sufficiently studied many of the results of your sagacity, ingenuity, and skill in experimental investigations. When I shall have time to make them the subject of the careful consideration which they merit, I may venture to subject your patience to the additional trial resulting from some further commentaries. I remain, with the highest esteem, respectfully yours,

ROBERT HARE."

In Faraday's answer to the preceding letter he said: ' You must excuse me, however, for several reasons, from answering it at any length. The first is my distaste for controversy, which is so great that I would on no account our correspond-

ence should acquire that character. I have often seen it do great harm, and yet remember few cases in natural knowledge where it has helped much either to pull down error or advance truth. Criticism, on the other hand, is of much value; and when criticism such as yours has done its duty, then it is for other minds than those either of the author or critic to decide upon and acknowledge the right.'

As late as November 30, 1844, Hare said, before the *American Philosophical Society*, "Faraday objects to the Newtonian idea of an atom, being associated with combining ratios. These he conceives to have been more advantageously designated as chemical equivalents.

This sagacious investigator adverts to the fact that after each atom in a mass of metal potassium, has combined with an atom of oxygen and an atom of water, forming thus a hydrated oxide, the resulting aggregate occupies much less space than its metallic ingredient previously occupied; so that taking equal bulks of the hydrate and of potassium, there will be in the metal only four hundred and thirty metallic atoms, while in the hydrate there will be seven hundred such atoms. And in the latter, besides the seven hundred atoms, in all two thousand eight hundred ponderable atoms. It follows that if the atoms of potassium are to be considered as minute impenetrable particles, kept at certain distances by an equilibrium of forces, there must be, in a mass of potassium, vastly more space than matter. Moreover, it is the space alone that can be continuous. The non-contiguous material atoms cannot form a continuous mass. Consequently the well known power of potassium to conduct electricity must be a quality of the continuous empty space, which it comprises, not of the discontinuous particles of matter with which that space is regularly interspersed. It is in the next place urged that while, agreeably to these considerations, space is shown to be a conductor, there are considerations



equally tending to prove it to be a non-conductor; since in certain non-conducting bodies, such as resins, there must be nearly as much vacant space as in potassium. Hence, the supposition that atoms are minute impenetrable particles, involves the necessity of considering empty space as a conductor in metals and as a non-conductor in resins, and of course in sulphur and other electrics. This is considered as a *reductio ad absurdum*. To avoid this contradiction, Faraday supposes that atoms are not minute impenetrable bodies, but, existing throughout the whole space in which their properties are observed, may penetrate each other. Consistently, although the atoms of potassium pervade the whole space which they apparently occupy, the entrance into that space of an equivalent number of atoms of oxygen and water, in consequence of some reciprocal reaction, causes a contraction in the boundaries by which the combination thus formed is inclosed. This is an original and interesting view of this subject, well worthy of the contemplation of chemical philosophers.

But upon these premises Faraday has ventured on some inferences which, upon various accounts, appear to me unwarrantable. I agree that "*a*" representing a particle of matter and "*m*" representing its properties, it is only with "*m*" that we have any acquaintance, the existence of "*a*" resting merely on an inference. Heretofore I have often appealed to this fact, in order to show that the evidence both of ponderable and imponderable matter is of the same kind precisely: the existence of properties which can only be accounted for by inferring the existence of an appropriate matter to which those properties appertain. Yet I cannot concur in the idea that because it is only with "*m*" that we are acquainted, the existence of "*a*" must not be inferred; so that bodies are to be considered as constituted of their materialized powers. I use the word materialized, because it is fully admitted by Faraday, that by dispensing with an

impenetrable atom "a," we do not get rid of the idea of matter, but have to imagine each atom as existing throughout the whole sphere of its force, instead of being condensed about the centre. This seems to follow from the following language:

*"The view now stated of the constitution of matter, would seem to involve necessarily the conclusion that matter fills all space, or at least the space to which gravitation extends, including the sun and its system, for gravitation is a property of matter, dependent on a certain force, and it is this force which constitutes matter."*

Literally this paragraph seems to convey the impression, that agreeably to the new idea of matter, the sun and his planets are not distinct bodies, but consist of certain material powers reciprocally penetrating each other, and pervading a space larger than that comprised within the orbit of Uranus. We do not live upon, but within the matter of which the earth is constituted, or rather within a mixture of all the solar and planetary matter belonging to our solar system. I cannot conceive that the sagacious author seriously intended to sanction any notion involving these consequences. I shall assume therefore, that, excepting the case of gravitation, his new idea of matter was intended to be restricted to those powers which display themselves within masses at insensible distances and shall proceed to state the objections which seem to exist against the new idea as associated with these powers.

Evidently the arguments of Faraday against the existence, in potassium and other masses of matter, of impenetrable atoms endowed with cohesion, chemical affinity, momentum, and gravitation, rest upon the inference that in metals there is nothing to perform the part of an electrical conductor besides continuous empty space. This illustrious philosopher has heretofore appeared to be disinclined to admit the existence of any matter devoid of ponderability. The



main object of certain letters which I addressed to him, was to prove that the phenomena of induction could not, as he had represented, be an "*action*" of ponderable atoms, but, on the contrary, must be considered as an *affection* of them consequent to the intervention of an imponderable matter, without which the phenomena of electricity would be inexplicable. This disinclination to the admission of an imponderable electrical cause, has been the more remarkable, as his researches have not only proved the existence of prodigious electrical power in metals, but likewise, that it is evolved during chemico-electric reaction, in equivalent proportion to the quantity of ponderable matter decomposed or combined.

According to his researches, a grain of water by electrolytic reaction with four grains of zinc, evolves as much electricity as would charge fifteen millions of square feet of coated glass. But in addition to the proofs of the existence of electrical powers in metals thus furnished, it is demonstrated that this power must be inseparably associated with metals, by the well known fact, that in the magneto-electric machine, an apparatus which we owe to his genius and the mechanical ingenuity of Pixii and Saxton, a coil of wire being subjected to the inductive influence of a magnet, is capable of furnishing, within the circuit which it forms, all the phenomena of an electrical current, whether of ignition, shock, or electrolysis.

The existence in metals of an enormous calorific power must be evident from the heat evolved by mere hammering. It is well known, that by a skillful application of the hammer, a piece of iron may be ignited. To what other cause than their inherent calorific power can the ignition of metals by a discharge of statical electricity be ascribed?

It follows that the existence of an immense calorific and electrical power is undeniable. The materiality of these powers, or of their cause, is all that has been questionable.

But, according to the speculations of Faraday, all the powers of matter are material; not only the calorific and electrical powers are thus to be considered, but likewise the powers of cohesion, chemical affinity, inertia and gravitation, while *of all these material powers only the latter can be ponderable!* ”

Thus a disinclination on the part of this distinguished investigator to admit the existence of one or two imponderable principles, has led him into speculations involving the existence of a much greater number. But if the calorific and electrical powers of matter be material, and if such enormous quantities exist in potassium, as well as in zinc and all other metals, so much of the reasoning in question as is founded on the vacuity of the space between the metallic atoms, is really groundless.

Although the space occupied by the hydrated oxide of potassium comprises two thousand eight hundred ponderable atoms, while that occupied by an equal mass of the metal, comprises only four hundred and thirty, there may be in the latter proportionably as much more of the material powers of heat and electricity as there is less of matter endowed with ponderability.

Thus while assuming the existence of fewer imponderable causes than the celebrated author of the speculation has himself proposed, we explain the conducting power of metals, without being under the necessity of attributing to void space the property of electrical conduction. Moreover, I consider it quite consistent to suppose that the presence of the material power of electricity is indispensable to electrical conduction, and that diversities in this faculty are due to the proportion of that material power present, and the mode of its association with other matter. The immense superiority of metals, as conductors, will be explained by referring to their being peculiarly replete with the material powers of heat and electricity.



Hence Faraday's suggestions respecting the materiality of what has heretofore been designated as the properties of bodies, furnish the means of refuting his arguments against the existence of ponderable impenetrable atoms as the basis of cohesion, chemical affinity, momentum and gravitation.

But I will in the next place prove, that his suggestions not only furnish an answer to his objections to the views in this respect heretofore entertained, but are likewise pregnant with consequences directly inconsistent with the view of the subject which he has recently presented.

I have said that of all the powers of matter which are, according to Faraday's speculations, to be deemed material, gravitation alone can be ponderable. Since gravitation, in common with every power heretofore attributed to impenetrable particles, must be a matter independently pervading the space throughout which it is perceived, by what tie is it indissolubly attached to the rest? It cannot be pretended that either of the powers is the property of another. Each of them is an "*m*," and cannot play the part of an "*a*," not only because an "*m*" cannot be an "*a*," but because no "*a*" can exist. Nor can it be advanced that they are the same power, since chemical affinity and cohesion act only at insensible distances, while gravitation acts at any and every distance, with forces inversely as their squares: and, moreover, the power of chemical affinity is not commensurate with that of gravitation. One part by weight of hydrogen has a greater affinity universally for any other element, than two hundred parts of gold. By what means then are cohesion, chemical affinity, and gravitation, inseparably associated, in all the ponderable elements of matter? Is it not fatal to the validity of the highly ingenious and interesting deductions of Faraday, that they are thus shown to be utterly incompetent to explain the inseparable association of cohesion, chemical affinity and inertia with gravitation; while the existence of a vacuity between Newtonian atoms, mainly relied upon as the

basis of an argument against their existence, is shown to be inconsistent both with the ingenious speculation, which has called forth these remarks, and those Herculean "researches" which must perpetuate his fame.

On the receipt of a pamphlet, entitled, "A Demonstration that All Matter is Heavy," from Prof. William Whewell of Cambridge University, Hare wrote (1842) the author as follows:

"Dear Sir:—I thank you for your kind attention in sending me a copy of your pamphlet entitled, "*A Demonstration that all Matter is Heavy*," comprising a communication made to the Cambridge Philosophical Society.

I conceive that to demonstrate that all matter is heavy, is, in other words, to prove that all matter is endowed with attraction of gravitation, or that general property which, when it causes bodies to tend towards the centre of the earth, is called weight. Hence to assert that all matter is heavy, is no more than to say, that attraction of gravitation exists between all or any masses of matter.

You say, "it may be urged that we have no difficulty in conceiving of matter which is not heavy." I have no hesitation in asserting, that there should be no difficulty in entertaining such a conception; since I cannot understand why any two masses may not be as readily conceived to *repel* as to *attract* each other, or *neither to attract nor to repel*. Is it not easier to imagine two remote masses indifferent to each other, than that they act upon each other? Is anything more difficult to understand than that a body can act where it is not?

It is also mentioned by you, that it may be urged "*that inertia and weight are two separate properties of matter*." Now I will not only urge, but also, with all due deference, will undertake to show, that the existence of inertia may as well be proven, and its quantity estimated, by means of repulsion as by means of attraction.



Suppose two bodies, A and B, to be endowed with reciprocal attraction; or, in other words, to gravitate towards each other. Being placed at a distance, and then allowed to approach, if, after any given time, it were found that they had moved severally any ascertained distances, evidently their relative inertias would be considered as inversely as those distances.

In the next place, let us suppose two bodies, X and Y, endowed with the opposite force of reciprocal repulsion, to be placed in proximity, and then allowed to fly apart. The distances run through by them severally, being, at any given time, determined, might not their respective inertias be taken to be inversely as those distances; so that the question would be as well ascertained in this case, as in that above stated in which gravitation should be resorted to as the test?

It seems to me that this question is sufficiently answered, in the affirmative, in your second paragraph, page 269, in which you allege, that "*one body has twice as much inertia as another, if when the same force acts upon it for the same time, it acquires but half the velocity. This is the fundamental conception of inertia.*"

In the third paragraph you say, "*that the quantity of matter is measured by those sensible properties of matter which undergo quantitative addition, subtraction, and division, as the matter is added, subtracted, or divided, the quantity of matter cannot be known in any other way; but this mode of measuring the quantity of matter in order to be true at all, must be true universally.*"

Also your fourth paragraph, fifth page, concludes with this allegation, "*and thus we have proved that if there be any kind of matter which is not heavy, the weight can no longer avail us, in any case to any extent, as the measure of the quantity of matter.*"

In reply to these allegations let me inquire, cannot a matter exist of which the sensible properties do not admit of

being measured by human means? Because some kinds of matter can be measured by "those sensible qualities which undergo quantitative addition, subtraction and division," does it follow that there may not be matter which is incapable of being thus measured? And wherefore would the method of obtaining philosophical truth be "futile" in the one case, because inapplicable in the other? Because the inertias of A and B have been discovered, by means of their gravitation, does it follow that the inertias of X and Y cannot be discovered by their self-repellent power? Why should the inapplicability of gravitation in the one case render its employment futile in the other?

It is self-evident, that matter without weight cannot be estimated by weighing, but I deny that on that account such weightless matter may not be otherwise estimated. The inertia of A and B cannot be better measured by gravitation than those of X and Y by repulsion, as already shown.

You seem to infer, in paragraph second, page sixth, that we should be equally destitute of the means of measuring matter accurately, "*were any kind of matter heavy indeed, but not so heavy, in proportion to its quantity of matter, as other kinds.*"

If in the case of all matter weight be admitted to be the only measure of quantity, it were inconsistent to suppose any given quantity of matter, of any kind; but upon what other than a conventional basis is it to be assumed, that there is more matter in a cubic inch of platinum than in a cubic inch of tin; in a cubic inch of mercury than in a cubic inch of iron? Judging by the chemical efficacy of the masses, although the weight of mercury is to that of iron as 13.6 to 8, there are more equivalents of the latter than the former in any given bulk, since by weight twenty-eight parts of iron are equivalent to two hundred and two parts of mercury.

Weight is one of the properties of certain kinds of matter, and has been advantageously resorted to, in prefer-



ence to any other property, in estimating the quantity of the matter to which it appertains. Nevertheless, measurement by bulk is found expedient or necessary in many cases. But may we not appeal to any general property which admits of being measured or estimated? Faraday has inferred that the quantity of electricity, is as the quantity of gas which it evolved. Light has been considered as proportional in quantity to the surface which it illuminates with a given intensity at a certain distance. The quantity of caloric has been held to be directly as the weight of water which it will render aeriform; and has also been estimated by the degree of its expansive or thermometric influence. What scale-beam is more delicate than the thermoscope of Meloni?

In the last paragraph but one, seventh page, you suggest that "*perhaps some persons might conceive that the identity of weight and inertia is obvious at once, for both are merely resistance to motion; inertia, resistance to all motion, or change of motion; weight resistance to motion upwards.*"

I am surprised that you should think the opinion of any person worthy of attention, who should entertain so narrow a view of weight, as antagonist of momentum, as that above quoted, "*that it is a resistance to motion upwards.*" Agreeably to the definition, given at the commencement of the letter, weight, in its usual practical sense, is only one case of the general force which causes all ponderable masses of matter to gravitate towards each other, and which is of course liable to resist any conflicting motion, whatever may be the direction. When in the form of solar attraction, it overcomes that inertia of the planets which would otherwise cause them to leave their orbits, does gravitation "*resist motion upwards?*"

In the next paragraph you allege, that "*there is a difference in these two kinds of resistance to motion. Inertia is instantaneous, weight is continuous resistance.*"

It is to this allegation I object, that as you have defined inertia to be "*resistance to motion, or to change of motion,*" it follows that it can be instantaneous only where the impulse which it resists is instantaneous. It cannot be less continuous than the force by which it is overcome.

Gravity has been considered as acting upon falling bodies by an infinity of impulses, each producing an adequate acceleration; but to every such accelerating impulse, producing of course a "*change of motion,*" will there not be a commensurate resistance from inertia? And the impulses and resistances being both infinite, will not one be as continuous as the other?

I have already adverted to inertia as the continuous antagonist of solar attraction in the case of revolving planets.

Agreeably to Mossotti, the creation consists of two kinds of matter, of which the homogeneous particles are mutually repellent, the heterogeneous mutually attractive. Consistently with this hypothesis, per se, any matter must be imponderable; being endowed with a property the very opposite of attraction of gravitation. This last mentioned property exists between masses consisting of both kinds of particles, so far as the attraction between the heterogeneous atoms predominates over the repulsion between those which are homogeneous. It would follow from these premises, that all matter is ponderable or otherwise, accordingly as it may be situated.

Can the ether by which, according to the undulatory theory, light is transmitted, consist of ponderable matter? Were it so, would it not be attracted about the planets with forces proportioned to their weight, respectively? and becoming of unequal density, would not the diversity in its density, thus arising, affect its undulations, as the transmission of sound is influenced by any variations in the density of the aeriform fluid by which it is propagated?

With esteem, I am yours truly,

ROBERT HARE."



Among the contributions made in 1847 by Hare was one entitled, "*On Free Electricity*," in which appear the following thoughts, disclosing the author's wonderful grasp of his subject:

"Practically there is a striking difference between the excitement of an electrified insulated conductor, the prime conductor of an electrical machine for instance, and the charge of a coated pane or Leyden jar. In the one case disruptive discharge is productive of a comparatively short thick spark, in the other of a spark distinguished by comparative length and tenuity. The discharge from the pan or jar is productive, for equal surfaces, of a much greater shock than could result from a spark ten times as long, from the conductor of the machine by which the electricity is generated. And yet if the intensity be inversely as the square of the striking distance, it must be a hundred times as great in the case of the conductor as in that of the coated surfaces.

Electricity, as it exists in the conductor, has been called free: as it exists about the coated pane, has been called simulated or disguised. Yet Faraday has alleged "that the charge upon an insulated conductor in the middle of a room, has the same relation to the walls of that room, as the charge upon the inner coating of a Leyden jar has to the outer coating of the same jar." "The one is not more dissimulated than the other." "As yet no means of communicating electricity to a conductor, so as to place its particles in relation to one electricity and not at the same time to the other, in an equal amount, has been discovered."

It seems to me that these opinions of Faraday have been judiciously criticized by Mr. Goodman in the London and Edinburgh Philosophical Magazine and Journal, Vol. XXIV, p. 174

It appears likewise that opinions harmonizing with those of Mr. Goodman, have been entertained by Charles V.

Walker, Hon. Sec. L. C. S., as may be seen in the Proceedings, Dec. 20, 1842. Agreeably to Mr. Walker, lightning resembles the discharge from a prime conductor, not that which takes place between the surfaces of a coated pane or jar.

I will proceed to state the considerations which induce me to concur in opinion with Mr. Goodman and Mr. Walker.

If two sufficiently remote insulated metallic disks, such as usually enter into the construction of an electrophorus, by due communication with the rubber and collecting points, be made to serve, one as the positive, the other as the negative conductor of an electrical machine in operation, a disruptive discharge from either may be obtained, by the approximation of an uninsulated conducting body, or one communicating with one conductor while approximated to the other. When this discharge takes place from a small knob on the positive side, to a large one on the negative side, of the circuit, the resulting spark is comparatively long, and by its zigzag form represents lightning in miniature.

If, in the next place, a sufficiently large pane of glass being interposed, the disks be made to serve as a coating to the glass, the surfaces of the pane which they touch will become oppositely charged. If immediately after the charging is effected, both disks being insulated, the knuckle of the operator, or any other conducting body in communication with the earth, be approached to either disk, a spark will pass, and on contact, a certain portion of electricity will be discharged. This is what I would call free electricity: but on making a conducting communication between the disks acting as coatings, a much larger discharge of electricity will take place. This is what I would call neutralized or dissimulated electricity. But the ratio in quantity of the latter to the former, varies evidently with the thickness of the pane or panes which may be interposed; so as to be inversely as the square of the distances of the charged surfaces. If a stratum of air per-



form the part allotted as above to the panes, the same law must hold good. But when instead of flat disks, corresponding in size and shape, we substitute a cylindrical or globular metallic mass, such as is generally used for the prime conductor of an electrical machine, on the one side, and on the other side the walls, floor, and ceiling of a room, for the other surface, evidently the ratio of the free electricity to that which can be neutralized must be enormously great. Supposing the glass pane to be one-tenth of an inch in thickness, the distance between the surfaces of the conductor and the parietes of the room to be ten feet, the quantity of electricity neutralized in the case of the pane will be to that neutralized in the case of the conductor as the square of one to the square of twelve hundred inversely; or in other words, nearly as a million and a half to one. It follows that in the phenomena of discharges from a prime conductor the neutralizing or dissimulating influence of the conducting superficies opposed to it must be too small to be regarded.

The allegation of Faraday, that no mode has been discovered by which to place the particles of a conductor in relation to one electricity, and not at the same time to the other, is verified, as Mr. Goodman has observed, when the friction between the rubber and glass takes place. The glass becomes positive to precisely the same extent as the rubber becomes negative; but when the vitreous surface thus excited moves away from the rubber, the compensating electricity of the rubber being no longer at hand, that upon the glass cannot realize Faraday's idea, excepting so far as it may be competent to act upon the walls, ceiling, and floor of the apartment, as electricity on the inner surface of a Leyden jar acts upon the outer surface. But in the case in point, the electric interposed is so enormously thick, compared with the glass in a Leyden jar, that very little of the inductive influence can avail to produce an opposite state tending to neutralize the electrical excitement "to an equal amount."

Just so far as it can produce an equivalent opposite state, it becomes dissimulated or neutralized; so far as it does not, it is free, or, in other words, exercises that uncompensated activity which has, in my opinion, justified the distinction made between free and dissimulated, neutralized, or latent electricity.

It will be perceived that I concur with Mr. Walker in the opinion, that on account of the distance of thunder clouds from the earth, the electricity which they may acquire is too remote from the terrestrial surface to induce in this an opposite electrical state, capable of neutralizing the electricity of the cloud beyond a minute proportion.

There seems to be an obvious means of discrimination between free and neutralized electricity, in the fact, that one is associated with the surface of a conductor, so as to accompany it when moved, while the neutralized electricity is inseparable from the superficies of the electric, through the intervention of which it exists. It is well known that the coatings of a pane or jar may be removed without disturbing the charge which may have been imparted by their presence. Yet if removed after the pane is fully saturated, each coating will hold a charge which it will give out in a spark to any uninsulated body, without any reference to the other coating which may meanwhile be remote and insulated from all communication with it. The spark thus yielded has the characteristics of free electricity. Having served as a part of the conductor, with which it had communicated, the coating is surcharged in proportion to its capacity, and gives up the redundancy on communicating with the earth, without any reference to the other coating. The spark thus given I conceive to have the characteristics of free electricity.

In the case of electric accumulations in the atmosphere, there can be no substitute for the service performed by glass in Leyden charges but that which air can render; and it can



hardly be conceived that while agitated, as it is during thunder gusts, a stratum of that fluid can perform the part of a glass pane."

Early in 1847 Hare adverted to his "hydro-oxygen blow pipe," particularly with reference to improvements in its construction and to the fusion of metals of the platinum group. He speaks of the "contrivance of two modes of producing a jet consisting of a mixture of hydrogen with oxygen. Agreeably to one mode, the gaseous currents meeting like the branches of a river, were made analogously to form a common stream. This object was accomplished by means of perforations drilled in a conical frustrum of pure silver, so as to converge until met by another shorter perforation, commencing at the opposite surface and so extended as to join them at the point of their meeting. The other mode was that of causing one tube to be within another, so as to be concentric; the outer tube being a little the longer of the two, the latter being employed for hydrogen, the other for oxygen."

It may suffice to add that the perfected apparatus enabled Hare to accomplish most remarkable results in the way of melting and purifying several of the platinum metals.

Now, we approach a momentous period. On Monday, May 10, 1847, Hare requested the Dean of his faculty (the medical) to convene his colleagues. At the ensuing meeting he announced his determination to resign his "situation as professor of chemistry," and at the same time desired the Faculty to consider the resignation as already made and to take action accordingly.

There is no known reason given for this step. He was in perfect sympathy with his immediate associates and with the governing Board. There is not anywhere a sign of dissatisfaction on either side, so that about the only conclusion at which one will arrive is that he had become weary of

the stupendous burden which he carried. His period in University service had been full of difficulties and trying labors. He, therefore, at the age of sixty-five concluded to retire. His going was most deeply regretted by his colleagues. In his absence they adopted the following resolutions unanimously:

1. *Resolved*, that the Medical Faculty in receiving notice of Dr. Hare's determination to resign his Professorship, retain the strongest sense of the zealous, unremitting and liberal efforts made by him, to render his branch efficient and instructive, and of the distinguished ability he has exhibited as a chemist;—also, that they have a most friendly recollection of the many gratifying circumstances arising from their connection with him, and the greatest regard for his high and honorable personal qualities.

2. *Resolved*, that the honorable Board of Trustees be respectfully requested to bestow upon Dr. Hare, as a mark of the just estimation, in which he is held in this Institution, and of his faithful services, the honorable title of Emeritus Professor of Chemistry.

Hare's letter of resignation, dated May 10, 1847, to the Dean of the Medical Faculty read:

I hereby tender my resignation of the Professorship, which for 29 years I have held under your auspices in the Medical Department, with a grateful sense of the kindness, which I have experienced from you individually as well as collectively.

I am

Yours respectfully,

ROBERT HARE."

This letter was transmitted to the Board of Trustees. At their meeting held May 15, 1847, the following resolution was unanimously adopted:

"Resolved, that in accepting the resignation of Dr. Hare, after an uninterrupted connection of 29 years, the Board



cannot refrain from expressing to him, their high regard for his character, their deep sense of the eminent services which he has rendered to science, and to the University of Pennsylvania, and their earnest wishes for his future happiness."

"Resolved that the appointment of Emeritus Professor of Chemistry be conferred upon Doctor Hare."

And thus passed from the University circle one of its most conscientious, devoted and eminent members—one of its most brilliant intellectual ornaments. His originality in thought and experiment was recognized everywhere throughout the learned and scientific world. He retired permanently. The only vestige now of his presence in the University is an old brass cannon used in demonstrating the explosibility of a mixture of hydrogen and air, or hydrogen and oxygen. For years the writer has insisted upon exhibiting this relic to his classes in elementary chemistry, largely because of his profound respect for the discoveries and personality of the subject of this biographical sketch. But the vast apparatus which astonished all who were so fortunate as to behold it, found place elsewhere (p. 214). Henceforth, the work of the renowned experimenter was to be carried forward in the laboratory in his own home.

## THIRD PERIOD

1847-1858

THIS, the shortest period in the life history of Robert Hare, is marked by variety. The labors attendant upon his professorship being now disposed of, he was free to occupy his time as he pleased. It is interesting to find that he very promptly addressed himself to a rather difficult problem, submitting his views as usual to the judgment of his constant friend Silliman, through whose Journal he then made his argument before the general public. This first communication as Professor Emeritus is highly speculative, elaborate and exhaustive. It bears the title, "Objections to the Theories Severally of Franklin, Du Fay and Ampère, with an Effort to Explain Electrical Phenomena by Statical or Undulatory Polarization." To it is appended a summary which may be here incorporated:

"The theories of Franklin, Du Fay and Ampère, are irreconcilable with the premises on which they are founded, and with facts on all sides admitted.

A charge of frictional electricity, or that species of electric excitement which is produced by friction, is not due to any accumulation, nor to any deficiency either of one or of two fluids, but to the opposite polarities induced in imponderable ethereal matter existing throughout space however otherwise void, and likewise condensed more or less within ponderable bodies, so as to enter into combination with their particles, forming atoms which may be designated as ethereo-ponderable.

Frictional charges of electricity seek the surfaces of bodies to which they may be imparted, without sensibly affecting the ethereo-ponderable matter of which they consist.

When surfaces thus oppositely charged, or in other words,



having about them oppositely polarized ethereal atmospheres, are made to communicate, no current takes place, nor any transfer of the polarized matter: yet any conductor touching both atmospheres, furnishes a channel through which the opposite polarities are reciprocally neutralized by being communicated wave-like to an intermediate point.

Galvano-electric discharges are likewise effected by waves of opposite polarization, without any flow of matter meriting to be called a current.

But such waves are not propagated superficially through the purely ethereal medium; they occur in masses formed both of the ethereal and ponderable matter. If the generation of frictional electricity, sufficient to influence the gold leaf electrometer, indicates that there are some purely ethereal waves caused by the galvano-electric reaction, such waves arise from the inductive influence of those created in the ethereo-ponderable matter.

When the intensity of a frictional discharge is increased beyond a certain point, the wire remaining the same, its powers become enfeebled or destroyed by ignition, and ultimately deflagration: if the diameter of the wire be increased, the surface proportionally augmented, enables more of the ethereal waves to pass superficially, producing proportionally less ethereo-ponderable undulation.

Magnetism, when stationary, as in magnetic needles and other permanent magnets, appears to be owing to an enduring polarization of the ethereo-ponderable atoms, like that transiently produced by a galvanic discharge.

The magnetism transiently exhibited by a galvanized wire, is due to oppositely polarizing impulses, severally proceeding wave-like to an intermediate part of the circuit where reciprocal neutralization ensues.

When magnetism is produced by a frictional discharge operating upon a conducting wire, it must be deemed a second-

ary effect, arising from the polarizing influence of the ethereal waves upon the ethereo-ponderable atoms of the wire.

Such waves pass superficially in preference; but when the wire is comparatively small, the reaction between the waves and ethereo-ponderable atoms becomes sufficiently powerful to polarize them, and thus render them competent, for an extremely minute period of time, to produce all the affections of a galvano-electric current, whether of ignition, of electrolysis or magnetization. Thus, as the ethereo-ponderable waves produce such as are purely ethereal, so purely ethereal waves may produce such as are ethereo-ponderable.

The polarization of hair upon electrified scalps is supposed to be due to a superficial association with the surrounding polarized ethereal atoms, while that of iron filings, by a magnet or galvanized wire, is conceived to arise from the influence of polarized ethereo-ponderable atoms, consisting of ethereal and ponderable matter in a state of combination.

Faradian discharges are as truly the effects of ethereo-ponderable polarization, as those from an electrified conductor, or coated surfaces of glass, are due to static ethereal polarization. . . .

It is well known that if a rod of iron be included in a coil of coated copper wire, on making the coil the medium of a voltaic discharge, the wire is magnetized. Agreeably to a communication from Joule, in the *L. and E. Phil. Mag. and Journal* for Feb., 1847, the bar is at the same time lengthened, without any augmentation of bulk; so that its other dimensions must be lessened in proportion to the elongation.

All these facts tend to prove that a change in the relative position of the constituent ethereo-ponderable atoms of iron, accompanies its magnetization, either as an immediate cause, or as a collateral effect."

Some of Hare's leisure was given to social intercourse, as appears from the following letter of 1848:



“Edgewood near Pelham Post Office

“My dear Silliman:      “Westchester 6 New York.

Mrs. Hare and myself are making a visit to our Daughter, Mrs. Prime. I should be glad to hear how you are, and how far you are capable of giving me some time should I pay you a visit, as New Haven is only two hours from this place by the rail way.

I should like to find your son at home if I am still to consider New Haven as his home. It is possible that Mrs. Hare either with or without me may make a trip to Niagara this summer and thence to Montreal and Quebec. We have also a visit to Miss Gibbs and to our son in Maryland in contemplation. You will perceive that our hands are full, or more properly our minds, for it is not always that we realize all we contemplate.

I sent to you a pamphlet some time since and hoped it reached you.      Yours faithfully

ROBERT HARE.”

It is further quite probable that it was during the visit just indicated that he wrote in great part or perhaps even completed a novel called “*Standish the Puritan*,” by Eldred Grayson, Esq., his pen name, although the book did not appear in print until 1850. It was published by Harper and Brothers of New York.

It is a tale of the American Revolution. It has as prominent characters, about whom the plot is mainly developed, three college class mates. They experience all sorts of adventures and changes which have been very interestingly depicted. The scenes are laid about, and not far from, New York City. The story occupies 320 pages.

In sketches of Hare, in encyclopedias, it is often said that he wrote frequently for the *Portfolio* under his pen name. Diligent search has been made in this publication,

but nothing of a certainty discovered. One or two stories seem to the writer to read as if they had emanated from Hare. There exist those ear-marks which would indicate this. However, it was thought best not to so regard them for fear of making a mistake or doing an injustice to another person.

There is nowhere any evidence of the manner in which Hare became interested in meteorological phenomena. However, it may be conjectured that his constant interest in natural phenomena and the presence of electrical conditions influenced him. Wherever electricity was a subject of discussion; wherever it entered—there it was pretty certain that his thought would be enlisted. This particular chapter in his scientific activities is somewhat remote from chemistry, but as physics also received his homage, these particular contributions should not be passed without consideration.

In a communication (1822),<sup>1</sup> his first probably, relating to meteorological matters, he discussed the north-east and north-west winds. To abridge this communication would be to mar its excellence, so it appears almost in extenso:

“Of the gales experienced in the Atlantic States of North America,” he said, “those from the north-east and north-west are by far the most influential; the one remarkable for its dryness; the other for its humidity. During a north-western gale, the sky, unless at its commencement, is always peculiarly clear, and not only water, but ice evaporates rapidly. A north-west wind, when it approaches at all to the nature of a durable gale, is always accompanied by clouds, and usually by rain or snow.”

For this striking diversity of character he accounts in this way:

“When to the lower strata of a non-elastic fluid, heat is unequally applied, the consequent difference of density (resulting from the unequal expansion,) soon causes movements,

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<sup>1</sup> Jr. Acad. Natural Sciences, Phila.



by which the colder portions change places with the warmer. These being cooled, resume their previous situation, and are again displaced by being again made warmer. Thus, the temperature reversing the situations, and these reversing the temperatures, a circulation is kept up tending to restore equilibrium. Precisely similar would be the case with our atmosphere, were it not an elastic fluid, and dependent for its density on pressure, as well as heat. Its temperature would be far more uniform than at present, and all its variations would be gradual. An interchange of position would incessantly take place, between the colder air of the upper regions, and the warmer, and of course lighter air near the earth's surface, where the most heat is evolved from the solar rays. Currents would incessantly set from the poles to the equator below, and from the equator to the poles above. Such currents would constitute our only winds, unless where mountains might produce some deviations. Violent gales, squalls, or tornadoes, would never ensue. Gentler movements would anticipate them. But the actual character of the air with respect to elasticity, is diametrically the opposite of that which we have supposed. It is perfectly elastic. Its density is dependent on pressure, as well as on heat, and it does not follow, that air which may be heated in consequence of its proximity to the earth, will give place to colder air from above. The pressure of the atmosphere varying with the elevation, one stratum of air may be as much rarer by diminution of pressure, consequent to its altitude, as denser by the cold, consequent to its remoteness from the earth, another may be as much denser by the increased pressure arising from its proximity to the earth, as rarer by being warmer. Hence when unequally heated, different strata of the atmosphere do not always disturb each other. Yet after a time, the rarefaction in the lower stratum, by greater heat, may so far exceed that in an upper stratum attendant on an inferior degree

of pressure, that this stratum may preponderate, and begin to descend. Whenever such a movement commences, it must proceed with increasing velocity; for the pressure on the upper stratum and of course its density and weight, increases as it falls; while the density and weight of the lower stratum, must lessen as it rises. Hence the change is, at times, so much accelerated as to assume the characteristics of a tornado, squall or hurricane. In like manner may we suppose, the predominant gales of our climate to originate. Dr. Franklin long ago noticed, that north-eastern gales are felt in the south-westernmost portions of the continent first, the time of their commencement being found later, as the place of observation is more to the leeward. This need not surprise us, as it is evident that a current may be produced either by a pressure from behind, or by a hiatus consequent to a removal of a portion of the fluid from before.

The Gulf of Mexico is an immense body of water, warm in the first place by its latitude, in the second place by its being a receptacle of the current produced by the trade winds, which blow in such a direction as to propel the warm water of the torrid zone into it, causing it to overflow and produce the celebrated Gulf Stream, by the ejection to the north-east, of the excess received from the south-east. This stream runs away to the northward and eastward of the United States, producing an unnatural warmth in the ocean, as well as an impetus, which according to Humboldt, is not expended until the current reaches the shores of Africa, and even mixes with the parent flood under the equator. The head of the Gulf Stream enables mariners to ascertain by the thermometer when they have entered it: and in winter this heat, by increasing the solvent power of the adjoining air, loads it with moisture, is precipitated in those well known fogs, with which the north-eastern portion of our continent, and the neighboring seas and islands, especially Newfoundland and its banks,



are so much infested. An accumulation of warm water in the Gulf of Mexico, adequate thus to influence the ocean at the distance of 2000 miles, may be expected in its vicinity to have effects proportionably powerful. The air immediately over the Gulf must be heated, and surcharged with aqueous particles.

Thus it will become comparatively light; first because it is comparatively warm, and in the next place because aqueous vapour, being much lighter than the atmospheric air, causes levity by its admixture.

Yet the density arising from inferiority of situation in the stratum of air immediately over the Gulf, compared with that of the volumes of the fluid lying upon the mountainous country beyond it, may to a certain extent, more than make up for the influence of the heat and moisture derived from the Gulf: but violent winds must arise so soon as these causes predominate over atmospheric pressure, so far as to admit the cold air of the mountains to be heavier.

When instead of the air covering a small portion of the mountainous or table land in Spanish America, that of the whole north-eastern portion of the North American continent, is excited into motion, the effects cannot but be equally powerful, and much more permanent. The air of the adjoining country first precipitates itself upon the surface of the Gulf, then that from more distant parts. Thus a current from the north-eastward is produced below. In the interim the air displaced by this current rises, and being confined by the high land of Spanish America, and in part possibly by the trade winds, from passing off in any southerly course, it is of necessity forced to proceed over our part of the continent, forming a south-western current above us. At the same time, its capacity for heat being increased by the rarefaction arising from its altitude, much of its moisture will be precipitated, and the lower stratum of the south-western current mixing with the upper stratum of the cold north-eastern

current below, there must be a prodigious condensation of aqueous vapour. If it be demanded, wherefore does this change produce north-eastern gales only, why have we not northern gales accompanied by the same phenomena? the answer is obvious. The course of our mountains is from the north-east to the south-west. Thus no channel is afforded for air proceeding to the Gulf in any other course than that north-eastern route which it actually pursues. The competency of the high lands of Mexico to prevent the escape over them of the moist warm air displaced from the surface of the Gulf, must be evident, from the peculiar dryness of their climate; and the evidence of Humboldt. According to this celebrated traveller, the clouds formed over the Gulf, never rise to a greater height than four thousand nine hundred feet, while the table land for many hundred leagues lies between the elevation of seven and nine thousand feet. Consistently with the chemical laws, which have been experimentally ascertained to operate throughout nature, air which has been in contact with water, can neither be cooled nor rarefied without being rendered cloudy by the precipitation of aqueous particles. It follows then, that the air displaced suddenly from the surface of the Gulf of Mexico, by the influx of cold air from the north-east, never rises higher than the elevation mentioned by Humboldt as infested by clouds. Of course, it never crosses the table land which at the lowest is 2000 feet higher.

Our north-western winds are produced, no doubt, by the accumulation of warm moist air upon the surface of the ocean, as those from the north-east are by its accumulation on the Gulf of Mexico. But in the case of the Atlantic, there are no mountains to roll back upon our hemisphere the air displaced by the gales which proceed from it, and to impede the impulse thus received, from reaching to the shores of Europe. Our own mountains may procrastinate the flood, and cause it to be more lasting and more terrific when it ensues. The



course of the wind is naturally perpendicular to the boundary of the aquatic region producing it, and to the mountainous barrier which delays the crises. The course of the North American continent is like that of its mountains, from north-east to south-west, and the gales in question are always nearly north-west, or at right angles to the mountains and the coasts. The dryness of our north-west may be ascribed not only to its coming from the frozen zone, where cold deprives the air of moisture, but likewise to the circumstance above suggested, that the air of the ocean is not like that of the Gulf, forced back over our heads to deluge us with rain.

Other important applications may be made of our chemical knowledge. Thus in the immense capacity of water for heat, especially when vapourized, we see a great magazine of nature provided for mitigating the severity of the winter. To cool this fluid, a much greater quantity of matter must be equally refrigerated. Aqueous vapour is an incessant vehicle for conveying the caloric of warmer climates to colder ones. Mistaking the effects for the cause, snow is considered as producing cold by the ignorant; but it has been proved that as much heat is given out during the condensation of aqueous vapour, as would raise twice its weight of glass to a red heat. Water, in condensing from the aeriform state, will raise ten times its bulk one hundred degrees. The quantum of caloric which can raise ten bulks 100 degrees, would raise one bulk 1000 degrees nearly (or to a red heat visible in the day) and this is independent of the caloric fluidity, which would increase the result.

Further, the quantum of heat which would raise water to 1000, would elevate an equal bulk of glass to 2000. Hence we may infer, that from every snow, there is received twice as much caloric as would be yielded by a like stratum of red hot powdered glass.

It is thus that the turbulent wave, which at one moment

rocks the mariner's sea-boat, on the border of the torrid zone, transformed into a cloud and borne away towards the arctic, soon after supports the sledge or the snow-shoe of an Esquimaux or Greenlander; successively cooling or warming the surrounding media, by absorbing or giving out the material cause of heat."

The next subject to engage his attention was tornadoes. In July of 1837 a terrific tornado occurred at Perth Amboy, N. J. Its effects had been observed by Profs. Henry, Torrey, Johnston, A. D. Bache and Espy. Hare himself visited the scene and wrote "after maturely considering all the facts, I am led to suggest that a tornado is the effect of an electrified current of air, superseding the more usual means of discharge between the earth and clouds in those sparks or flashes which are called lightning. I conceive that the inevitable effect of such a current would be to counteract within its sphere the pressure of the atmosphere, and thus enable the fluid, in obedience to its elasticity, to rush into the rare medium above.

It will, I believe, be admitted that whenever there is sufficient electricity generated to afford a succession of sparks, the quantity must be sufficient under favorable circumstances, to be productive of an electrical current; and that light bodies, lying upon one of the electrified surfaces may be attached more or less by the other."

And of the tornado which in August, 1838, passed over Providence, R. I., he concluded its characteristics to be quite similar to those of the tornado which had previously fallen upon New Brunswick, N. J.; and that they fully justified his "opinion that the exciting cause of tornadoes is electrical attraction." The *tornado* in Philadelphia, on July 13, 1840, was due in his opinion to electricity as the principal reagent in the production of the observed phenomena. And in a verbal communication before the American Philosophical Society (1840) he told how Peltier, commenting on a tor-



nado which visited Paris in July of that year, said a tornado is a thunder gust in which the electricity, instead of appearing as lightning, passed through a cloud, acting as a conductor between the earth's surface and the sky. This view differed little from that submitted some time before by Hare to the Society. The only difference, said Hare, was "that the Parisian philosopher omitted to call in the electricity of the air in the aid of the electrical forces, and his assigning to a cloud the agency which he (Hare) had attributed to a vertical blast of electrified air, mingled with every species of movable matter coming within the grasp of the meteor." Hare contended that Peltier had misapprehended his theory. This he thought probably due to Peltier's ignorance of English. He further said:

"During an examination of the track of the tornado which ravaged the suburbs of New Haven, he had been led to infer that the electrical discharge was concentrated upon particular bodies, according to their character, or the conducting nature of the soil; so that the vertical force arising from electrical reaction, and the elasticity of the air, acted upon them with peculiar force. Hence, while some trees were borne aloft, others, which were situated very near them, on either side, remained rooted in the soil. In two instances at New Haven, wagons were especially the victims of the electro-aerial conflict. In the case of one of these, the axle-tree was broken, and while one wheel was carried into an adjoining field, the other was driven with so much force against the weather-boarding of a barn, as to leave both a mark of the projecting hub, and of the greater portion of the periphery. The plates of the elliptical spring were separated from each other. During the tornado at New Brunswick, the injury done to some wagons in the shop of a coach-maker, appeared, at the time, inexplicable. Now he inferred, that the four iron wheel tires, caused, by their immense con-

ducting power, a confluence of the electric fluid, producing a transient explosive rarefaction, and a subsequent afflux of air with a local gyration of extreme violence.

It may be reasonably surmised, that the excessive injury done to trees results, not from the general whirl, but from a local gyration to which they are subjected, in consequence of the multiplicity of points which their twigs and leaves furnish for the emission of the electrical fluid. The fact that the leaves of trees thus injured, appear afterwards as if they had been partially scorched, seems to countenance this idea. The twisting of the chimney at New Brunswick, seems difficult to explain, agreeably to the idea of a general whirl throughout the whole area of the tornado track. The chances are infinitely against any chimney having its axis to coincide with that of a great whirlwind, forming a tornado; and it must be evident, that in any other position, it could only be subjected to the rotary force on one side at a time. But if this were adequate to twist the upper upon the residual portion, the former would necessarily be overthrown. Evidently, it could not be left, as was the chimney.

During the tornado at New Haven, chimneys seemed to be especially affected. One, after being lifted, was allowed to fall upon a portion of the roof of the house to which it belonged, at a distance from its previous situation too great to have been reached had it been merely overthrown. In the case of a church which was demolished, a portion of the chimney was carried to a distance greater than it could have reached without being lifted by a vertical force.

It appeared quite consistent that chimneys should be particularly assailed, since that rarefaction, which, by operating upon the roofs of houses, carries them away, must previously cause a great rush of air through the chimney flues. But this concentration of the air must tend to facilitate the "convective" discharge in that direction, since an elec-



trical discharge by a blast of air, is always promoted by any mechanical peculiarities favouring an aerial current, or jet.

That in the tornado in France, articles were carried from the inside of a locked chamber to a distance without, when no opening existed besides that afforded by a chimney, seemed to justify the suggestion, that there must be a great rush of air through such openings.

Hare also remarked on the aurora which occurred on the third of September, in which he suggested that the electric fluid producing the phenomena then observed might have been derived from remote parts of space.

Hare was so much interested in the tornado problem that he had translated his own views into a communication that he sent to each member of the National Institute. This he did to show that Peltier's ideas were essentially identical with his own and that Peltier was incorrect in declaring Hare's hypothesis as defective.

Again Hare reported on the effect of the rarefaction of air, on its desiccation and refrigeration, and on other phenomena connected with the presence of aqueous vapour in the atmosphere. He also detailed some experiments, showing that the phenomena of air, heated by re-entering a receiver partially exhausted, were more consistent, in some respects, with the idea that a vacuum has a capacity for heat, than that it is destitute of any appropriate portion of caloric.

He adverted to the fact, that in an essay, published in 1822, he had, agreeably to the authority of Dalton and Davy, stated, that the cold consequent on the rarefaction of air in its ascent towards the upper strata of the atmosphere, was one of the causes of the formation of clouds; and in his text books he had soon after published an engraving of an apparatus, by means of which he was accustomed to illustrate, before his pupils, the transient cloud which arises from a diminution of pressure in air containing aqueous vapour.

He had alleged, that as much caloric was given out by aqueous vapour, during its conversion into snow, as would be yielded by twice the weight of red-hot powdered glass. But Mr. Espy, he considered, had the merit of being the first to suggest, that the heat, thus evolved, might be an important instrument in causing a buoyancy tending to accelerate any upward current of warm moist air.

Hare was willing to admit, that this transfer of heat might co-operate with other causes in the production of storms, but could not concur with Espy in considering it competent to give rise to thunder gusts, tornadoes, or hurricanes. These he had considered, and still considered, to be mainly owing to electrical discharges between the earth and the sky; or between one mass of clouds and another.

With a view to a more accurate estimate of the comparative influence or rarefaction and condensation, in causing evolution of heat in dry air, and in air replete with aqueous vapour, he had performed a number of experiments, of which he proceeded to give a description.

Large globes, each containing about a cubic foot of space, furnished with thermometers and hygrometers, were made to communicate, respectively, with reservoirs of perfectly dry air, and of air replete with aqueous vapour. The cold, ultimately acquired by any degree of rarefaction, appeared to be the same, whether the air was in the one state or the other; provided that the air, replete with aqueous vapour, was not in contact with liquid water in the vessel subjected to exhaustion. When water was present, in consequence of the formation of additional vapour, and a consequent absorption of caloric, the cold produced was nearly twice as great as when the air was not in contact with liquid water; being nearly as 9 to 5.

Under the circumstances last mentioned, the hygrometer was motionless; whereas, when no liquid water was accessible, the space, although previously saturated with vapour, by the



removal of a portion of it together with the air which is withdrawn by the exhaustion acquires a capacity for more vapour; and hence the hygrometer, by an abstraction of one-third of the air, resolved more than sixty degrees towards dryness. But when a smaller receiver (after being subjected to a diminution of pressure of about ten inches of mercury, as to cause the index of the hygrometer to move about thirty-five degrees toward dryness) was surrounded by a freezing mixture, until a thermometer in the axis of the receiver stood at three degrees below freezing, the hygrometer revolved towards dampness, until it went about ten degrees beyond the point at which it rested when the process commenced.

It appears, therefore, that the dryness produced by the degree of rarefaction employed is more than counterbalanced by a freezing temperature.

As respects the heat imparted to the air above mentioned, the fact, that the ultimate refrigeration in the case of air replete with vapour, and in that of anhydrous air, was equally great, and that when water was present the cold was greater in the damp vessel, led to the idea, that the heat arising under such circumstances could not have much efficacy in augmenting the buoyancy of an ascending column of air; but when, by an appropriate mechanism, the refrigeration was measured by the difference of pressure at the moment when the exhaustion was arrested, and when the thermometer had become stationary, it was found *caeteris paribus*, that the reduction or pressure arising from cold was at least one-half greater in the anhydrous air, than in the air replete with vapour. This difference seems to be owing to a loan of latent heat made by the contained moisture, or transferred from the apparatus, by its intervention, which checks the refrigeration; yet ultimately, the whole of the moisture being converted into vapour, the aggregate refrigeration does not differ in the two cases. . . .

When air, replete with aqueous vapour, was admitted into a receiver partially exhausted, and containing liquid water, a copious precipitation of moisture ensued, and a rise of temperature greater than when perfectly dry air was allowed to enter a vessel containing rarefied air in the same state. In the instance first mentioned, a portion of vapour rises into the place of that which is withdrawn during the partial exhaustion. Hence when the air, containing its full proportion of vapour, enters, there is an excess of vapour which must precipitate, causing a cloud, and an evolution of latent heat from the aqueous particles previously in the aeriform state. Hare conceived that as the enlargement of the space occupied by a sponge, allows, proportionably, a larger quantity of any liquid to enter its cells, so any rarefaction of the air when in contact with water, consequent on increase of heat or diminution of pressure, permits a proportionably larger volume of vapour to associate itself with a given weight of the air. When, subsequently, by the afflux of wind replete with aqueous vapour, the density of the aggregate is increased, a portion of the vapour equivalent to the condensation must be condensed, giving out latent heat, excepting so far as the heat thus evolved, being retained by the air, raises the dew point.

Hence, whenever a diminution of density of the air inland causes an influx of sea air to restore the equilibrium, there may result a condensation of aqueous vapour, and evolution of heat, tending to promote an ascending current. This process being followed by that which Espy pointed out, of the transfer of heat from vapour to air, during its ascent to the region of the clouds, and consequent precipitation of moisture, might, Hare thought, be among the efficient causes of those non-electrical rain storms, during which the water of the Gulf of Mexico, or of the Atlantic, is transferred to the soil of the United States.



Hare mentioned some additional experiments made by him respecting the increase of temperature resulting from the admission of dry air into an exhausted receiver. When the receiver was exhausted so as to reduce the interior pressure to one-fourth of that of the atmosphere, and one-fourth was suddenly admitted, so as to reduce a gauge from about  $22\frac{1}{2}$  inches to 15 inches, heat was produced; and however the ratio of the entering air to the residual portion was varied, still there was a similar result.

When the cavity of the receiver was supplied with the vapour of ether or with that of water, so as to form, according to the Daltonian hypothesis, a vacuum for the admitted air, still heat was produced by the latter, however small might be the quantity, or rapid the readmission. When the receiver was exhausted, until the tension was less than that of aqueous vapour at the existing temperature, so as to cause the water to boil, as in the Cryophorus, or Leslie's experiment, still the entrance of  $\frac{6}{1000}$  of the quantity requisite to fill the receiver caused the thermometer to rise a tenth of a degree. An alternate motion of the key of the cock, through one-fourth of a circle, within one-third of a second of time, was adequate to produce the change last mentioned.

He considered the fact, that heat is produced, when to air, rarefied to one-fourth of the atmospheric density, another fourth is added, irreconcilable with the idea, that this result arises from the compression of the portion of air previously occupying the cavity, since the entering air must be as much expended as the residual portion is condensed.

As, agreeable to Dalton, a cavity occupied by a vapour acts as a vacuum to any air which may be introduced, Hare argued, that when a receiver, after being supplied with ether or water, is exhausted so as to remove all the air and leave nothing besides aqueous or ethereal vapour, the heat, acquired by air admitted, cannot be ascribed, consistently, to the condensation of the vapour.

These facts, he added, are not reconcilable with the idea of De la Rive and Marcet, that the first portion of the entering air is productive of cold, although a subsequent condensation is productive of an opposite change. The effect upon the thermometer was too rapid, and the quantity of the entering air too minute, to allow it to be refrigerated by rarefaction in the first place, and yet afterwards to be so much condensed as to become warm by the evolution of caloric.

Notwithstanding the experiments of Gay Lussac and of those of De la Rive and Marcet, there appeared to him to be evidence in favour of the heat being due to the space, rather than to the air which it contained.

With respect to Gay Lussac's celebrated experiment with the Torricellian vacuum, supposing such a vacuum to be a pre-eminently good liberator of heat, as it ought in reason to be, the caloric would be absorbed by the mercury as rapidly as this metal could be made to encroach upon the space occupied by the calorific particles.

Admitting that, for equal weights, the specific heat of air is seven times as great as that of mercury, there could not have been a capacity greater than that of about 200 grains of the metal, whereas a very small stratum of this metal, equal to one-fourth of an inch would, in the apparatus employed, amount to more than a pound.

The rapidity with which a mercurial thermometer is affected by the changes of temperature, in experiments like those which he had been describing, showed in Hare's opinion, that there was something not yet understood respecting the transfer of heat in such cases. It was hardly reconcilable with the process of conduction or circulation, as ordinarily understood.

In the experiments of De la Rive and Marcet, in which the entering air being made to impinge upon the bulb of a thermometer, was productive of a fall in the thermometric



column, it might be inferred, he conceived, that the bulb interfered with the access of caloric from the space. It was in fact the bulb upon which the air acted previously to its distribution in the space where it could have encountered the due proportion of caloric."

At another time he referred to observations on the suspension of clouds "made by me last summer (1841) in Switzerland," when he gave as his opinion that "clouds were constantly forming and dissolving masses of vapour." He remarked that "although there were occasionally two different sets of clouds pertaining severally to different currents of air, one above the other,—usually, in fair weather, there was but one set. In either case all the clouds belonging to one current are seen to be situated somewhere between two levels. Above the space, included between these levels, none are seen to rise; nor are any observed to sink below its lower boundary. It was conceived that the causes of this persistence of the clouds between two horizontal planes, of which the lower one is usually more than a mile in height, had never been satisfactorily assigned.

Agreeably to the prevalent impression that clouds are enduring masses of condensed aqueous vapour, their specific gravity ought to be much greater than that of the subjacent cloudless air, over which they swim; since the little watery bubbles of which they are formed, consist, not only of the air with which they are inflated, but also of a liquid 840 times as heavy. But he had of late years observed that clouds are not as durable as generally supposed. On the contrary, like the steam condensed in escaping from boiling water, they are incessantly forming by the condensation of aqueous vapour, and disappearing in consequence of its being vaporized again. A cloud may appear to cling to the brow of a mountain, sometimes for more than an hour; when, on closer examination, it may be discovered that, as one portion appears,

another vanishes, and that the apparent durability is due to the equality of the causes of condensation and revaporization. He had enjoyed a fine opportunity of verifying this view of the subject, when involved within a cloud on the summit of the Rhigi. It was quite evident, that what might, at a distance, be mistaken for an enduring mass of condensed moisture, such as is called a cloud, was really due to a current of air, saturated with aqueous vapour, which was rushing up the mountain side. As this current reached a level at which the temperature was below its dew point, the contained vapour was converted by condensation into a cloud; but as it attained a higher level, where the dew point was sufficiently low to compensate for the cold, the moisture was made to resume the aeriform state.

As in condensing, steam relinquishes as much heat as would make it red-hot, if retained while under sufficient pressure to keep it in the liquid state, it follows that, as the cloud is formed, the temperature of the air with which it is associated is raised so much as to produce a buoyancy which enables it to float or even to ascend; but as soon as it reaches a point where the air is so devoid of aqueous vapour as to permit it to be revaporized, a proportionable refrigeration and increase of density ensues. Thus the buoyancy produced at one level, is compensated by a commensurate opposite influence at another. Of course, the clouds are always seen to occupy an interval between two horizontal planes, one above the other. As soon as the aqueous vapour of the air rises above the lower plane it condenses; before the cloud thus produced can get beyond the upper one it is reconverted into vapour.

When the causes of condensation are more potent than those of revaporization, rain ensues; when the opposite is the case, there must be a tendency to fair weather.

Although of opinion that in hurricanes and other violent



rain storms, there must be an exchange of position between the lower and upper strata of the air, he conceived that showers, unaccompanied by gales or squalls, were to be explained as above suggested.

He conceded that there might be more than one cause for the buoyancy of clouds, for Thomson, he said, in his treatise respecting Heat and Electricity, suggested electricity as a cause. The fact demonstrated by the experiment, the results of which had been communicated to the Society, that moisture does not render air a conductor of electricity, gives support to this view of the subject; especially since it has been discovered, that in condensing, steam becomes highly electrified. It seems inevitable that the aqueous globules, of which clouds are constituted, must separate from each other, as pith balls are seen to do when similarly excited; and that the particles of air with which they are associated must be similarly actuated; hence a cause of rarefaction, and of course of buoyancy. Another cause might co-operate. It is known that radiation of heat, which causes dew and sometimes hoar-frost, is so completely checked by clouds, that the last mentioned phenomenon never takes place when the sky is overcast. Moreover, it is known that the solar rays pass through the air without imparting heat, until intercepted by solids or liquids. It follows that the air in which clouds are situated, will be warmer than that above and below them.

Thus radiant heat and electricity may promote their buoyancy; nevertheless their persistency between two levels must be ascribed to the process noticed on the summit of the Rhigi.

Espy had the merit of drawing the attention of meteorologists more strongly to the fact, previously made known by Dalton that, although cold is produced by the rarefaction of air containing vapour, yet the reduction of temperature is less, whenever the vapour is condensed, than it would have been in an air free from vapour.

In adopting this explanation Hare had been prompted by his knowledge of Espy's suggestions founded on those of Dalton, so far as a superior temperature had been ascribed to the air containing a recent cloud.

In the year 1842 Redfield entered upon a study of storms. He maintained "that tornadoes and hurricanes are all whirlwinds." Hare contended that grave improbabilities were herein involved, because in explaining them by reference to the "simple conditions of the great law of gravitation," the agency of electricity is neglected, and "the theory of calorific rarefaction" was renounced. Hare declared that gravitation "in lieu of being . . . the main basis of winds and storms, tends to produce that equal distribution of the atmosphere over the surface of the globe on which I have insisted." He then proceeds, "but if neither gravity, nor calorific expansion, nor electricity, be the cause of winds, by what are they produced?" Redfield replied that all fluid matter has a tendency to run into whirls or circuits, when subject to the influence of unequal or opposing forces; and that, in this way, a rotative movement of unmeasured violence is sometimes produced. But, argued Hare, if this be true, plainly whirlpools or vortices of some kind, ought to be as frequent in the ocean, as agreeably to your observation, they are found to be in the atmosphere. . . . There are few vortices or whirlpools in the ocean, because there are in very few cases ascending currents, to supply which the confluence of the surrounding water is requisite. . . . The conflict of opposing or unequal forces does not produce curvilinear motion unless there be a successive deflection . . . and Redfield does not tell us how these unequal or opposing forces are generated in the atmosphere. He simply appeals to "*certain unequal or opposing forces by which a rotative movement of unmeasurable violence is produced;*" this rotative movement, although alleged to be an effect in the first in-



stance, is later said to be "the only known cause of violent and destructive winds or tempests." Then Hare reiterates his oft-declared statement "that the proximate cause of the phenomena of a tornado is an ascending current of air, and the afflux of wind from all points of the compass to supply the deficiency thus created." In this view he and Espy agreed, but differed "respecting the cause of the diminution of atmospheric pressure within the track of the tornado, which gives rise to the ascending current." Hare regarded gyration as a casual, not an *essential feature* "in the meteors in question." Espy and Bache had recorded a fact irreconcilable with a general whirling motion, and Hare cited "the statement of a most respectable witness, that while the tornado at Providence was crossing the river, the water which had risen up as if boiling within a circle of about three hundred feet, subsided as often as a flash of lightning took place. Now supposing the water to have risen by a deficit of pressure resulting from the centrifugal force of a whirl, how could an electrical discharge cause it to subside?" And Hare continues: "I have already, I trust, sufficiently shown that the explanation which Redfield dignifies with the title of his "theory of rotary storms," amounts to no more than this, that certain imaginary nondescript unequal and opposing forces produce atmospheric gyrations, that these gyrations by their consequent centrifugal force, create about the axis of motion a deficit of pressure, and hence the awful and destructive violence displayed by tornadoes and hurricanes.

I cannot give to this alleged theory the smallest importance, while the unequal and opposing forces, on which it is built, exist only in the imagination of an author who disclaims the agency either of heat or electricity." . . .

I cannot help thinking that as respects the application of his "rotary theory" to account for the upward movement which appears to be essential to tornadoes, these arguments will amount to a "*reductio ad absurdum*." . . .

So far, therefore, as Redfield's observations confirm the idea that the whirling motion in tornadoes quickens towards the centre, it tends to confirm the opinions which he combats, and to refute those which he upholds.

Although the efforts which I have made to show that the phenomena of tornadoes and hurricanes arise from electrical reaction should not be successful, I think it will be conceded that any theory of storms which overlooks the part performed by electricity must be extremely defective.

Both by Messrs. Espy and Redfield the influence of this agent in meteorological phenomena is entirely disregarded, although with the storms which have been especially the subject of their lucubrations, thunder and lightning and convective discharge are most strikingly associated."

Redfield, of course, replied and said that, among other things, the pains Hare had taken to confute his doctrines were disproportional to the low estimation in which he professed to hold them; but Hare proceeded to narrowly study his reply, saying "the author alleges that in the absence of reliable facts and observations" in support of my objections to what he considers as the "established character of storms," he had hesitated to answer them. This cannot excite surprise, when it is recollected "that the whole modern meteorological school," and likewise "Sir John Herschel," are accused by him of a "*grand error*," in not ascribing all atmospheric winds "*solely to the gravitating power as connected with the rotary and orbital motion of the earth.*"

For this denunciation he has no better ground than that on which he deems his theory to be above my reach, that is to say, because himself and others have made some observations showing that in certain storms, agreeably to log-book records, certain ships have had the wind in a way to indicate gyration. Being under the impression, that in many instances no better answer need be given to Redfield's opinions



than that created in the minds of scientific readers by his own language, I will here quote his denunciation of the opinions of the meteorological school and of Herschel.

“The grand error into which the whole school of meteorologists appears to have fallen, consists in ascribing to heat and rarefaction the origin and support of the great atmospheric currents which are found to prevail over a great portion of the globe.” . . . “An adequate and undeniable cause for the production of the phenomena . . . I consider is furnished in the rotative motion of the earth upon its axis, in which originate the centrifugal and other modifying influences of the gravitating power, which must always operate upon the great oceans of fluid and aerial matter, which rest upon the earth’s crust, producing of necessity those great currents to which we have alluded.” . . . Speaking of Sir John Herschel’s explanation of the trade winds and others, Redfield alleges, “Sir John has however erred, like his predecessors, in ascribing mainly, if not primarily, to heat and rarefaction those results which should have been ascribed solely to mechanical gravitation as connected with the rotative and orbital motion of the earth’s surface.”

Is it not surprising, asks Hare, that it did not occur to the author of these remarks, that an astronomer so eminent as Sir John Herschel would be less likely than himself to be ignorant of any atmospheric influence resulting from gravitation or the diurnal and annual revolutions of our planet—and that when he found himself in opposition to the whole school of meteorologists, a doubt did not arise whether the “*grand error*” was not in his views of the subject instead of that which they had taken?

Redfield alleges “in his reply to my objections that it is an error to consider him as rejecting the influence of heat.” It is very possible that his opinions may have changed since he read my “objections”; but that he did reject the influence

of heat when the preceding and following opinions were published must be quite evident.

Mr. Redfield alleges further that the proper enquiry is What are storms? not How are storms produced?

Turning from an endless controversy with a writer with whom I differ respecting first principles, I shall address myself to that great school of meteorologists who concur with me in the "grand error" of considering heat and electricity as the principal agents of nature in the production of storms, and who do not concur with Redfield in considering gravitation and the earth's annual and diurnal motion as the great destroyer of atmospheric equilibrium. So far as it may conduce to truth, I shall incidentally notice some parts of Redfield's reply; but my main object will be to show the inconsistency of his theoretic inferences with the laws of nature, and the facts and observations on which those inferences are alleged to be founded. To follow him in detail through all the misunderstandings which have arisen, and which would inevitably arise during a continued controversy, would be an Ixion task.

I do not deem it expedient to enter upon any discussion as to the competency of the evidence by which the gyration of storms has been considered as proved. By Espy that has been ably contested. I have given some reasons for doubting the accuracy or consistency of Redfield's representations, though I have no doubt they have always been made in perfect good faith. I have already alleged, that were gyration sufficiently proved, I should consider it as an effect of a conflux to supply an upward current at the axis. Yet the survey of the New Brunswick tornado, made on *terra firma*, with the aid of a compass, by an observer so skillful and unbiased as Professor Bache, ought to outweigh maritime observations, made in many cases under circumstances of difficulty and danger. In like manner great credit should be given to the observations collected by Professor Loomis



respecting a remarkable inland storm of December, 1836."

"Having said so much against the whirlwind theory of storms, it may be expected that I should on this occasion, say something respecting the opinions which I entertain of their origin. To a certain extent this will be found in my communications published in the *Am. Jr. Science*, Vol. XXXII, p. 153, Vol. XL, p. 137, also in my essay on the gales of the United States (p. 445). I still believe that north-eastern gales were correctly represented in the last mentioned essay as arising from an exchange of position made between the air of the Gulf of Mexico and that of the territory of the United States which lies to the north-east of that great estuary; and that the heat given out during the conversion of aqueous vapour into rain, by imparting to the atmosphere as much caloric as could be yielded by twice its weight of red hot sand, is a great instrument in the production of the phenomena; also, that the cold resulting from rarefaction is a cause of the condensation of that vapour, and of course of clouds. On this last idea, derived from Dalton, Mr. Espy has founded his ingenious theory of storms; alleging, erroneously, as I think, the buoyancy resulting from the heat thus evolved, to be the grand cause of rain, also of tornadoes, hurricanes, and other electrical storms. I did err in ascribing too much to variations of density arising from changes of elevation, and twenty years' additional experience as an experimenter in electricity, has taught me to ascribe vastly more to this agent than I did formerly. To pursue this subject fully, would give this paper an undue length. . . .

As bodies oppositely electrified attract each other, "a fortiori," attraction must always exist between any bodies sufficiently electrified for an electric discharge to take place between them. Hence the rising of the water within the track of a tornado and its subsidence on the passage of lightning, as observed by Mr. Allen, near the city of Providence,

R. I., may be considered as resulting from the alternation of convective with disruptive discharge. By this observation of Mr. Allen, attraction is shown to have existed between an electrified stratum of air coated by clouds, and the oppositely electrified water of a subjacent river. It is reasonable to infer that attraction, originating in the same way, operating upon the denser stratum of the atmosphere in the vicinity of the earth, by counteracting gravitation may cause that rarefaction by which houses are burst or unroofed, and an upward current of tremendous force produced. We may also infer that bodies are carried aloft by the joint action of the electrical attraction and the vertical blast which it produces.

The effects upon the leaves noticed by me after the tornado of New Brunswick in 1835, and still more those subsequently observed by Peltier after that of Chaenaye in 1839, cannot be explained without supposing them to have been the medium of an electric discharge.

Any heat imparted to air in rising from the terrestrial surface to the region of the clouds, by the condensation of aqueous vapour, being applied to the upper part of the column and rendering it as much taller as lighter, cannot speedily make its total weight less than that of the surrounding air, and must therefore be insufficient to cause any violent changes, like those which constitute tornadoes or hurricanes, as argued by Espy. Moreover, the process on which so much stress has been laid by this ingenious meteorologist, cannot generate rain storms during which the rain freezing as it falls, the temperature of the lower stratum is shown to be below the freezing point of water, while that of the upper stratum, within which water condenses in the liquid form, must be above that point.

Were the causes assigned by Espy adequate to create a tornado or hurricane, a storm of this kind would exist incessantly in the vicinity of the equator, where in consequence



of the never ceasing ascent of warm moist air from the ocean, that afflux of this fluid from neighboring regions takes place, to which the trade winds are attributed.

Experience has demonstrated that electricity cannot exist on one side of an electric, without its existence simultaneously on the other side. If the interior of a hollow globular electric be neutral so will the outside be; but if the interior be either positively or negatively electrified, the outside will be found in the one case positive, and in the other negative.

The atmosphere is an electric in a hollow globular form, and as electricity is known to pervade the space within it occupied by earth, the principle in question must also pervade the space beyond that portion of the atmosphere which is sufficiently dense to insulate, or to perform the part of an electric.

Thus there are three enormous concentric spaces, of which the intermediate one is occupied by an electric, while the innermost one and the outer one are occupied by conductors. The two last mentioned, may be considered as equivalent to two oceans of electricity, of which one may be called the celestial, the other the terrestrial electric ocean. For an adequate cause of diversity in the states of the electric oceans, it must be sufficient to refer to the vapourization and condensation of water. The power of this process to electrify has recently been confirmed by the electrical sparks caused by the escape of high steam.

When either electric ocean is minus the other must be plus, and at the same time any intermediate stratum of the atmosphere enclosing a stratum of clouds, must be charged by induction if not by communication. Between the concentric strata of air, severally bounding the celestial and terrestrial ocean, there must be an electrical attraction tending to counteract gravitation and thus to influence the density and pressure of the lower stratum of the atmosphere.

The proximity of a stratum of clouds electrified by the

celestial ocean, must cause an accumulation of electricity in any portion of the terrestrial surface immediately subjacent; and by counteracting gravitation, cause a local diminution of atmospheric pressure which is, it is well known, a precursor and demonstrably a cause of wind and rain.

Those enormous discharges of electricity which take place during hurricanes, may be accounted for by supposing that they result from discharges between the celestial and terrestrial electric oceans. Thunder clouds may owe their charges not only to the vapourization and condensation of water, but also to the celestial ocean previously charged by that process. Auroras may be the consequence of discharges from one part of the atmosphere to another, through the rare conductive medium which is occupied by the celestial ocean; or they may result from discharges from other planets or suns, or from any part of space however remote. Since, agreeably to Wheatstone's experiments, electricity flies with a velocity not less than that of light, distance can create no obstacle to its passage.

In November last, subsequently to the submission of the opinions above expressed to the Academy of Natural Sciences, I verified a conjecture of my friend, Dr. F. K. Mitchell, that moist, foggy or cloudy air is not a conductor of electricity, its influence, in paralyzing the efficacy of electrical apparatus, arising from the moisture deposited on adjoining solid surfaces.

A red hot iron cylinder, upon which evidently, no moisture could be deposited, suspended from the excited conductor of an electrical machine, was found to yield sparks within a receiver replete with aqueous vapour, arising from a capsule of boiling water.

Hence it appears that bodies of air, whether cloudy or clear, may be oppositely electrified, from each other or from the earth. This would explain the gyration on a horizontal



axis which seems to be attendant on thunder gusts, and may account for the ascent of the southeaster and descent of the northwester in the great storm of December, 1836, described by Loomis.

Such gyration may be a form of convective discharge, in which electrical reaction is assisted by calorific circulation and the evolution of latent heat, agreeably to Dalton and Espy.

Squalls may be the consequence of electrical reaction between the terrestrial surface and oppositely excited masses of air, and the intermixture of masses so excited, in obedience to the same cause, may be among the sources of rain, hail, and gusts. The specific gravity of a body of air, electrified differently from the surrounding medium, may be lessened by what is called electric repulsion; the particles inevitably moving a greater distance from each other, as similarly electrified pith balls are known to do.

Hence a cause of rarefaction, buoyance, and consequent upward motion, in a column of electrified air, more competent than that suggested by Espy.

Should it be verified that a gyration from right to left takes place, during convective discharges of electricity in hurricanes, it may be referrible to the disposition which a positive electrical discharge from the earth to the sky would have to gyrate in that direction."

Hare never did admit the rotary theory of Redfield. The latter would never consent to an oral controversy with Hare at the meetings of the American Association for the Advancement of Science, where much of this material was presented. Those present at the second meeting of the Association, held in New Haven, said they would never forget "the zeal and energy with which Dr. Hare, in an off-hand speech, fluent and animated, assailed the views of Mr. Redfield, who was all the while a quiet and silent listener. The responses of the

latter were always made by the pen and never on public occasions by the tongue."

Dove also discussed "The Law of Storms." This called forth a vigorous protest (1843) from Hare. Is there not he asked:

"a great mistake made by Redfield and other advocates of the whirlwind theory; in treating gyratory motion as a *cause of violence*. . . . ? I have not been able," he continues, "to discover that Dove attempts to assign any cause for violent winds." . . .

"I would recommend Loomis's observations to the candid attention of Dove, and would request him to show in what manner the earth's motion co-operated to produce it; or how the enormous length of the focal area, or area of minimum pressure, comparatively with its breadth, can be reconciled with the idea of its having formed the centre of an extensive whirlwind. There is another fact which would seem to be literally an unsurmountable obstacle to the rotation of a storm travelling from the valley of the Mississippi to the Atlantic coast. I allude to the interposition of the Alleghany mountains. Dove's imaginary aerial cylinder would be cut nearly in twain when bestriding that range. Obviously more than one half of the air in such a cylindrical mass would be below the average level of the summits of those mountains. Under such circumstances could it be conceived to rotate about a vertical axis?

I am aware that various writers have referred to the little transient whirls which are occasionally seen to take place in windy times, carrying up dust, leaves, and other light bodies, as a support for the idea of whirlwind storms; and Redfield has alleged, "that no valid reasons can be given why larger masses of air may not acquire and develop similar rotative movements."

It appears to me that there are several valid reasons for



not adopting the view of the subject which he has taken. The momentum by which any body is kept in motion, is as its weight multiplied by its velocity, while the expenditure of momentum in *cæteris paribus* as its surface. On this account, a globe of which the content in proportion to its superficies is pre-eminently great, will, in a resisting medium like the air, retain a rotary motion longer than an equal weight, of the matter forming it, in any other shape. The flat cylinder, in diameter about two hundred times its thickness, of which the existence would be necessary to an extensive whirlwind, is a form of which the surface would be very great in proportion to the quantity of matter it contains. No observer ever noticed any whirl produced as above described, to have a diameter many times greater than its height, or to endure many minutes. Such pigmy whirls appear to be the consequence of eddies resulting from the conflict with each other or with various impediments, of puffs or flaws of wind. No doubt, in this way a deficit of local density is easily caused in a fluid so elastic as the air, and consequently by gravity as well as its elastic reaction, a centripetal motion is induced in the surrounding aerial particles. From the confluence and conflict of the air thus put into motion, a whirl may arise. The manner in which light bodies are gathered towards the axis of these whirls, shows that they are accompanied by a centripetal tendency. It is only when the wind blows briskly that such whirls are ever seen to take place, but tornadoes agreeably to universal observation occur when there is little or no wind externally.

According to the evidence adduced by the advocates of the whirlwind theory, there is in this respect perfect similarity in the phenomena of tornadoes and hurricanes. Beyond the sphere of the alleged gyration, there is but little if any atmospheric commotion, and certainly none competent to be the cause of a great whirlwind. It follows that pigmy whirl-

winds and hurricanes can have no analogy. The former are never produced without a proportionable external activity in the wind, while comparative external quiescence seems to accompany the latter.

I will conclude by applying to Dove the stricture which I applied, on a former occasion, to Espy, and to Redfield. He has, I think, committed a great oversight in neglecting to take into consideration the agency of electricity in the generation of storms."

Espy, believing that Hare's strictures upon Dove's statements were in reality an attack upon his own ideas published quite a spirited reply, concluding:

"To me it appears, that the main course of discussion pursued by Hare in one hundred and twenty-eight elaborate paragraphs, is essentially misapplied and erroneous. If the supporters of a rotative or whirlwind action in tornadoes and hurricanes had chosen to maintain their cause in a speculative manner, the case might have been different. But when their facts and results were offered on the basis of direct observations, which had been set forth, in many cases, with particularity and precision, it seems like a waste of words to assail these observed phenomena and results with strictures and objections of this character; volumes of which can never equal in value the direct observations which may be made of the phenomena of a single storm."

In 1852 Hare published (Weed, Parsons & Co., Albany) "Strictures on Prof. Espy's Report on Storms." He concurred with Espy "in the influence that hurricanes and tornadoes are the consequence of the ascent of air from a focal area or intermediate space, by which a confluence from two or more opposite quarters, to supply the deficit thus arising, is induced," but he differed with him as to the cause of the ascent of air in such cases. It will be recalled that as early as 1835, before the American Philosophical Society, Hare



had based the ascent in question to a discharge of electricity between the earth and the sky. Indeed, in 1836 he published a memoir on this explanation in the Transactions of the Society, and now in this reply to Espy he reviews his own views, giving them more briefly and forcibly than he had done before.

On June 3, 1852, Mr. John Wise made his 131st balloon ascension. He proceeded from Portsmouth, Ohio. He encountered and was in three distinct storms. His report of the conditions about him and the atmospheric changes in particular attracted Hare's attention. Reviewing all most carefully, he, at great length, discussed details (1854) and demonstrated "that they are quite consistent with the idea that electricity is a principal agent in the generation of storms."

A memoir on the explosion of nitre was published by Hare in 1849. From it we learn that on July 19, 1845, a great fire took place in the city of New York. The phenomena attending it were awful and mysterious. Two hundred and thirty houses were destroyed. These contained merchandise to the value of two millions of dollars. From a series of detonations noted by every person it was assumed that gun-powder was the cause, but the "oaths of worthy and well informed persons" indicated that no gun-powder was contained in the building within which the explosions occurred. The real cause of the disaster became a subject of perplexing consideration for chemists. It was fully established "that there were in the store more than 300,000 pounds of nitre, secured in double gunny bags, containing one hundred and eighty pounds of nitre each, in piles alternating with heaps of combustible merchandise." In the opinion of Silliman, Hayes and other eminent chemists, the unfortunate results were to be due possibly "to the reaction of the nitre with contiguous merchandise."

Upon approaching Hare on the subject he recalled that upon one occasion "a mischievous explosion had occurred in my laboratory, when a fissure taking place in an iron alembic holding about twenty pounds of fused nitre, on hoisting the alembic off the fire, a jet of the liquefied salt fell accidentally upon some water in a tub, which was unfortunately too near." It also occurred to him that potassium when thrown upon the surface of water, is, by combustion with the oxygen of that liquid, converted into "a fused globule of red hot oxide, which, in the act of combining with water, detonates violently."

He said further that in the winter of 1845-1846, "I found that when nitre, by the flame of a hydro-oxygen blow-pipe supplied with atmospheric air and oxygen, is heated to incandescence, and then quickly submerged in water previously situated beneath the containing ladle, a sharp explosion ensues. . . . I have fallen upon contrivances, by which pulverized sugar and nitre may be made to explode. The first expedient which succeeded, was that of pouring melted sugar upon the face of a hammer, so as to make a disk of commensurate size. . . . Some nitre was put into a thin shallow platina capsule, situated over a small anvil, near one of its edges, so that the bottom of the capsule might be reached obliquely by a hydro-atmospheric blow-pipe flame. Under these circumstances, the nitre having been heated until its potash began to be volatilized, was struck with the sugar-faced hammer. A smart detonation was the consequence. . . ."

"Another method of producing explosive reaction is as follows:—Nitre and sugar being coarsely powdered, let disks of paper about three inches in width, be prepared. Place one of the disks upon an anvil, and cover it with a stratum of sugar. Then cover the sugar with a stratum of nitre, placing over this another of the disks. Heat a flat iron bar, wider than the disks, to a welding heat, and quickly with-



drawing it from the fire, and holding it above the paper, strike it down thereon with a sledge. An explosion will ensue, with a very loud report. . . .”

“Having submitted the preceding facts and considerations, my explanation of the stupendous explosion which forms the topic of this communication is as follows:

Of the enormous quantity of nitre which the store held, more than 56,000 pounds were on the first floor, about 180,000 pounds on the second floor, and about 100,000 on the third floor. The weight of combustible merchandise was about 700,000 pounds. As it was alleged by some of the witnesses examined that the iron window shutters of an upper story became red hot by the conflagration of an adjoining house, it is probable that fire was communicated to some of the gunny bags holding the nitre, or some other combustibles, which, as stated in evidence, were piled against the shutters. As soon, however, as a single bag became ignited, the nitre with which the inner bag must have been imbued, would give the greatest deflagrating intensity to the consequent combustion; while the interstices between the bags, like those between grains of gunpowder, would enable the flame to pervade the whole heap of bags. As nitre fuses at a low red heat, very soon a great quantity, in a state of liquefaction, must have run down upon the wooden floor, which would immediately burst into an intense state of reaction with the oxygen of the salt. To this combustion the merchandise adjoining would add fuel, causing a still more extensive liquefaction of the nitre. The deflagrating mass thus created, on burning its way through the floor, or falling through the scuttles, which were all open agreeably to the evidence, must have received an enormous reinforcement from the subjacent nitre or combustible merchandise. On the giving way of each floor in succession, the conflagration must have received a reinforcement of deflagrating fuel, so as to have grown rapidly with its growth,

and strengthened with its strength. Under these circumstances, the whole of the nitre becoming liquefied, must have found its way to the cellar. Meanwhile, the merchandise and the charcoal of the wood-work must have been conglomerated by the fusibility of the sugar, shellac, and bitumen, aided by the molasses, and formed thus an antagonistic mass of more than half a million of pounds in weight, deflagrating intensely with the nitre. But, whenever, by these means, a portion of the deflagrating congeries attained the fulminating temperature, a detonation must have ensued, causing a temporary lifting of the combustible mass; only, however, to be followed by a more active collision, resulting from the subsequent falling back of the conglomerated combustible mass upon the melted nitre. After every such collision, the combustible congeries must have been blown up to a height augmenting with the temperature, the force of the fall, and extent of reciprocal penetration. The force of the fall would, of course, be as the height. Hence the twelve or thirteen successive detonations indicate as many explosive collisions; while the successive augmentation of the loudness of the reports indicates a proportionable growth of their violence, arising from successively greater elevation and descent."

And it was with reference to the preceding that he wrote Dr. Franklin Bache:

"Lynwood Ellicots Mills Md.

"July 25th, 1850.

"My dear Sir

I owe you my acknowledgements for your letter of the 23rd which is quite satisfactory. Since I wrote to you I have found that an extract from my communication respecting the explosiveness of nitre had been published in the North American. When my memoir now in the Press is published I hope you will give it some attention and when we meet we can talk the matter over. The improvements in using the



gas from the public works will be best appreciated when I am enabled to show the phenomena and the results.

I remain with esteem

Yours sincerely

“Dr. Franklin Bache.”

ROBERT HARE.”

“I have concluded to avail myself of this letter to draw your attention to two facts which serve to illustrate the influence of chemical affinity as a substitute for mechanical compression.

A globule of oxide of iron falling through a subjacent stratum of water may be seen for a short time ignited beneath it on the supporting shelf of the pneumatic cistern without any explosive reaction with the water; while under similar circumstances a globule of oxide of potassium (potassa) explodes as soon as the oxidation producing it ceases.

A globule of any volatile liquid may be supported in proximity with the surface of an incandescent candle by generated vapour without exploding when on quicklime (or any other refractory surface capable of absorbing or coalescing with the liquid or either of its ingredients under the actual circumstances) explosion will cease.

Explosive mixtures such as can be made with nitre and chlorate of potassa deflagrate without exploding when unconfined, doing no harm to the supporting body. But explosive chemical compounds such as the fulminates of silver or mercury, argentate or aurate of ammonia, the chloride of nitrogen, or perchloric ether, fracture the vessels in which they may be exploded and do not in any case deflagrate.”

In 1854 we find Hare, in pamphlet, defending one Barker against the attacks of a Rev. Dr. Berg. It is, in brief, a series of objections to existing sectarianism. Its contents are evident from the following closing paragraphs:

“To conclude, my object in this publication, has been to

resist the effort which has been long and strenuously made, in this, as well as in other parts of Christendom, to represent believers in God, as adopting their opinions from bad motives, ('baseness of heart' as recently alleged at Concert Hall.) This unprincipled method of sustaining orthodoxy, so-called, is productive of great injustice and oppression to many who, like myself, consider a book of no higher authority than the fallible men who wrote it. Moreover, this intolerance gives rise to hypocrisy arising from the fear of persecution affecting a man in business or any pursuit in which he may want influence or popularity. Against this tyranny I conceive it to be my duty to stand up in defence of my brethren in opinion who may be less independently situated.

I am the more encouraged to take this stand, because in my opinion, and that of many others, I have, of late, had positive scientific proof of a future state; in which, without any reference to scripture, a position is given to souls proportional to their merit, independently of faith either in Christ, Mahomet, Moses, or Bhuda."

It may have been that this occurrence prompted him to write:

Did not that thought from Heav'n proceed,  
According God's mercy to every creed,  
Howe'er pagan, howe'er untrue,  
If it meant to give our Creator his due?  
May not devotion to God be shown,  
Whether through Crist or through Mohadded known?  
Whether men die in holy war,  
Or kneel to be crushed by Juggernaut's car?  
Mankind would God in error leave,  
Yet penalty for that error aggrieve.  
Did God a special creed require,  
Each soul would he not with that creed inspire?

Can a glaring evil endure  
Despite of the power and will to cure?



Must not any event arrive  
 For which both will and power strive?  
 Will not any result obtain  
 Which power unites with will to gain?  
 If God can creatures make to suit his will,  
 Forsee, if they can, his design fulfill;  
 Wherefore to trial those creatures expose,  
 Traits to discover, which he thus foreknows?

In November, 1855, he appeared in New York City before an audience of more than three thousand persons with a "Lecture on Spiritualism," in which he set forth the facts which induced his "conversion to spiritualism and confirmed his hope of immortality."

He said: "A practical man, with whom I had become acquainted . . . urged me to investigate the manifestations; saying I was in error in assuming that the tables moved by the aid of mortals, since he had seen them move without visible contact by any person. . . . A friend, offering to take me to a circle, I went, . . . I was all vigilance—a thorough unbeliever, earnestly hoping that I should obtain an explanation agreeably to the received laws of nature . . . determined to prevent the possibility of deception, I constructed . . . the *spiritoscope*."

It was by this instrument that he claimed he was able to have interviews with his father, with Washington and with Franklin, who approved of a recent theory of electricity which he had enunciated.

During this year he also issued, through a New York publishing house, a volume of 460 pages, bearing the title:

EXPERIMENTAL INVESTIGATION  
 OF THE  
 SPIRIT MANIFESTATIONS

The following paragraph from the preface may well be pondered by those who have followed Hare in his earnest

search for truth in the fields of chemistry and physics and who have been impressed by the originality of his inquiries and the profundity of his knowledge:

“Those who shall give a careful perusal to the following work will find that there has been some ‘method in my madness’; and that if I am a victim to an intellectual epidemic, my mental constitution did not yield at once to the miasma. But let not the reader too readily ‘lay the flattering unction to his soul’ that “ ’tis my hallucination that is to be impugned, not his ignorance of facts and his educational errors.”

By means of the spiritoscope, the inquirer was able to communicate directly with “spirits” and not need the presence of a *medium*. This instrument Hare offered to exhibit before a Convention of clergymen of his own faith, but the offer was refused. It was in the letter addressed to them that the following lines appeared:

However late, as holy angels teach,  
Souls now in Hades, bliss in Heaven may reach.  
All whose conduct has been mainly right,  
With lightning speed may gain that blissful height;  
While those who selfish, sensual ends pursue,  
For ages may their vicious conduct rue,  
Doom'd in some low and loathsome plane to dwell,  
Made through remorse and shame the sinner's hell;  
Yet through contrition and a change of mind,  
The means of rising may each sinner find.  
The higher spirits their assistance give,  
Teaching the contrite how for Heaven to live.

At Albany, at a meeting of the American Association for the Advancement of Science, he was permitted, after much opposition, in deference solely to his age and to his reputation as a scientist, to read an elaborate article on spiritualism, which did not appear in its Transactions.

In a volume entitled “*Psychic Facts*,” published in London as late as 1880, Hare's experiments in spirit manifesta-



tions find place with articles contributed by Sir William Crookes, C. F. Varley, Zöllner of Leipzig, Alfred Russell Wallace, Lord Lindsay, Butlerof and others eminent in various walks of life.

Stray letters addressed to Dr. John F. Frazer, editor of the *Franklin Journal*, have come to hand. Their exact connection with Hare's work is, except in one case, uncertain. In the instance of the one which follows, it is safe to conclude that it refers to the contents of the paper of 1857. Several attempts to exhibit the apparatus mentioned had previously failed.

" My dear Sir

Prof. Henry has authorized me to exhibit the apparatus for showing the nature of the phenomena of the function of quartz before the Franklin Institute at the expense of the Smithsonian Institute—

If therefore there is no objection on the part of the former I will direct Mr. Wight to transfer the apparatus forthwith.

Yours truly,

Prof. Frazer

ROBERT HARE "

Wednesday

26th Feb<sup>y</sup>

1851 "

I presume on notice those interested attend on any evening mentioned."

In 1857 Dr. Robert Hare exhibited before the members of the Franklin Institute an apparatus (referred to in preceding letter) for ascertaining whether the phenomena attending the attrition of pieces of quartz, when rubbed briskly together, had anything in common with a supposed new body described by Schönbein under the name of ozone. When the apparatus was in operation, scintillations and the odor, which was the object of the inquiry, resulted. The Doctor

remarked: "In no way, however, could I produce the chemical effect of ozone upon iodized starch or guiacum tincture. On directing a jet of hydrogen between the stones it took fire forthwith, but I could not by means of a gold leaf electrometer detect any electricity." The scintillations and odor had been produced by rubbing against each other two pygmy mill stones of seven inches made of cellular horn stone. The stones were, in successive experiments, made to revolve *in vacuo*, in hydrogen, and in a vacuity previously repleted with this gas without any diminution of the illuminating phenomena. These, it seems from the injection of the jet of hydrogen, constitute a simple case of ignition. The concentration of the frictional force and the transparency of the mass under which the ignition is effected make the corruscations very brilliant in a room otherwise darkened. "It has long since occurred to me that the phenomena of light under all the various hues which it is capable of producing are ascribed to the undulatory affections of the ether pervading the universe, so that the still greater variety of odors which influence our olfactory nerves may be due to vibratory agitation of the same medium. If odors are to be ascribed to ethereal affections produced by impulses proceeding from odoriferous substances, consequently taste must have an analogous origin, and mesmeric influence, so far as its existence has been proved, seems equally to require ethereal intervention. It may be conceived that the odor produced during ozonification during the attrition of quartz is due to an odoriferous ethereal affection."

Evidently there must have been a disposition on the part of persons connected with medical education to depreciate the value of chemistry as a fundamental in such training, otherwise Hare would not have felt constrained to submit the following opinions to a Medical Convention held in the year 1857:



“ The knowledge requisite to a medical education cannot be thoroughly acquired either by the study of books on the one hand, or by attendance on demonstrative courses on the other: both of these means of improvement being requisite to the education of a competent physician.

Pathology, therapeutics, surgery, materia medica, and midwifery, are of the most immediate importance to the healing art, chemistry and anatomy being useful only so far as they are subservient to the branches thus enumerated.

Nevertheless, as chemistry and anatomy are among the fundamental branches of medical science, any attempt to give a medical education in which they should be neglected, would be like attempting to erect a superstructure without a basement.

Of the branches requisite to medical graduation, those are most necessary to be taught by lectures in a medical school, which require experimental or demonstrative illustration; and, consequently, the branches of anatomy and chemistry, being pre-eminently susceptible of this assistance, are among those which are most important to be taught by the lecturers in schools of medicine. So far as materia medica can be accompanied by an exhibition of plants, minerals, and pharmaceutical preparations, and so far as surgery and midwifery require and admit of operative illustration, these are next in order to anatomy and chemistry in their claims to be *lectured* upon. Further, so much of the practice and the institutes as can be assisted by clinical lectures, have also pretensions, in this way, of the highest order; but to the extent that such branches are insusceptible of illustration to the eye of the student, they may be as well if not better learned, by reading, than by listening to lecturers.

Hence, so far as the time which medical students can be made to give their attendance on medical schools is insufficient to enable them to listen to all the branches of medical

science which are therein taught, it is important that they should be required to attend those lecturers *preferably*, of whose instruction there will be the least subsequent opportunity to repair the neglect.

As chemistry has become more and more important in its services to physiology and materia medica, in proportion as it has become more abstruse and more extensive, it were wrong to indulge that increasing indolence and disgust which no small proportion of medical students display, as respects the augmented study and attendance on lectures requisite to acquire a due knowledge of this wonderful science.

Although in the superb column constituted by a thorough medical education, practice and the institutes form the capital, outranking, thus, all other constituents, yet they cannot exist without their subordinates. Hence any effort to impart the knowledge requisite to form an accomplished physician, by lecturing on pathology, therapeutics, and physiology, without a concomitant, if not a previous effort to teach fundamental branches, were like placing workmen upon a scaffold, to carve the entablature of a column before completing the pedestal.

The increased difficulty of acquiring a knowledge of chemical science, arising from its miraculous progress, ought not to justify its neglect; but on the contrary, greater efforts should be made, both as respects the means of experimental illustrations, and in lecturing on this highly important branch of medical knowledge.

Under these circumstances, those who are authorized to grant medical degrees, ought not to leave it to the option of the students, whether or not to be ignorant of chemistry.

Since chemistry is becoming an object of study with the intelligent agriculturists of the United States, it must have an unfavorable influence upon the estimation in which physicians will be held, if farmers should, in chemical science,



become their superiors; so that, on inquiry, they should be found ignorant of the nature of the earth on which they tread, of the food which they eat, of the air which they breathe, of the medicines which they prescribe, or of the flesh and bones composing the animal frame, which is the object of their skill; yet such is believed to be the ignorance of a large portion of those who now annually receive the honours of a medical diploma.

The preceding suggestions being duly considered, the hope is entertained that the medical profession will feel it to be their duty, to use all their influence to induce the medical schools of the country to deny a medical diploma to those whose knowledge of chemistry is below mediocrity.

Evidently, if chemistry be requisite to medicine, a knowledge of it should be enforced; if not requisite, it should be omitted from the list of sciences necessary to the acquisition of a medical diploma."

The importance accorded chemistry in medicine to-day would no doubt have delighted Hare's heart! He saw its paramount benefits but was not spared to realize its recognition.

It was in 1857 or 1858 that Hare, before the American Association for the Advancement of Science, proposed a plan for the making of small weights. He said:

"In chemical analysis, and in the assay of the precious metals, the accuracy of the extremely minute weights employed is of the utmost importance.

As the government has undertaken to furnish standard weights and measures for the larger operations or transactions of commerce and the arts, without which accuracy and uniformity could not be secured to the country at large, so it would seem consistent that to the minute processes of the arts and sciences a help should be given which otherwise seems not to be attainable.

The usual process of making weights, by reducing them till they exactly counterpoise a standard weight, cannot be pursued advantageously when they are less each than a tenth of a grain. For the making of weights below that size, measurement and division are preferably employed.

An instrument constructed by an ingenious and skillful machinist (Tyler) is capable of dividing an inch into 1,400 parts by the action of a ratchet and wheel, which may be so restricted in its motion as only to move one tooth at a stroke, causing a platform to advance only the fraction of an inch above mentioned.

For producing by means of this instrument tenths and hundredths of a grain, a convenient length of very fine palladium wire may be employed. This being reduced in length very cautiously till it weighs some equimultiple of a grain, a distance commensurate with the length of this wire is marked upon a suitable narrow brass plate by a knife. The number of the ratchet strokes which must be made in order to measure this distance must be ascertained.

Dividing this number by the number of grains will give the number of ratchet movements in a length of the wire equal in weight to a grain; again dividing this by ten will give the number of such movements in a length equal to a grain; and, in like manner, if divided by one hundred will give the number of the movements in question requisite to designate the length equal to  $\frac{1}{1,000}$  of a grain. The length equal to as much of the wire as would weigh a tenth of a grain being thus found, this distance is to be marked on the brass plate with a sharp edge.

A strip of steel is in the next place sharpened at each end to a fine edge, and bent so as to resemble a long narrow staple, is to be furnished midway with a screw, by which the ends can be made nearer or farther apart, like the points of spring compasses. By these means, with the aid of a lens, the edges



of the tool thus constructed are to be made to coincide exactly with the marks designating the length of wire equal to the tenth of a grain.

Having made this adjustment by the action of the tool, ten pieces of the wire being cut and afterwards weighed against a standard grain weight, if found too light or too heavy, the screw regulating the distance must be touched so as to cause the distance to be increased or diminished, rendering the cuts larger or smaller. When they are brought to the weight required, they should, by a delicate assay balance, be tried against each other to ascertain that they are equal in weight to each other.

Having thus obtained tenths of a grain in weight equal to each other, fifths may be made by the same process and tried against the tenths, two to one, and against each other; hundredths may be obtained by a like process; for each a tool being requisite like that used for cutting tenths, excepting that it should be smaller in proportion as the lengths required to be cut are shorter.

The instrument by which these results were obtained has a peculiar capability of reducing the size of the graduations to the limits requisite to include a greater or less number within any necessary length.

Suppose it desirable to have as much of a rod as would be equivalent in bulk to a cubic inch of water divided into such degrees as would increase hundredths of a cubic inch. Let a tube sufficiently large to receive the whole length of the rod be at one end recurved at right angles, and terminated in a point with a capillary orifice. Let the other end be furnished with a stuffing-box to receive the rod, making a water-tight juncture. The tube is to be replete with water, the rod entering so as to reach a little beyond the stuffing-box. A mark is then to be made on the rod as close to the box as possible. A light cup being counterpoised on an ac-

curate balance, and a weight equal to a cubic inch of water being placed in the opposite scale, the apex of the rod is to be introduced into the cup while the rod is shoved in, until as much water has been forced into it from the tube as will balance the weight employed as above mentioned. Another mark is now to be cut into the rod close to the box as before. Thus a length of the rod equivalent to the water excluded, and of course equal to a cubic inch of that fluid, is thus indicated to exist between the knife marks.

The number of ratchet strokes requisite to measure this distance is in the next place to be ascertained and divided by the number of graduations required. The quotient will be the number necessary to make a degree.

Should the number of the ratchet strokes, when decided as above mentioned, leave a fraction, it is to be gotten rid of by means of the contrivance already alluded to for reducing each degree proportionally to any reduction in the whole length necessary to a degree.

I am willing to put the instrument in question into the possession of the government or of the Smithsonian Institution on condition that it shall be kept in good order for the purpose of furnishing accurate weights, graduations or measures of liquids or gases, for the purposes of science and the arts."

The writer recalls reading a perfectly charming letter from Louis Agassiz written in the summer of 1858, to a distinguished man of science in Philadelphia, relative among other things to the formation of an Academy of Science. Agassiz advised that it should consist of sections; "that two members of each Section should be selected to begin the elections—the two best men beyond question . . . ! The number of members in each Section to be limited. . . . Would Henry and Hare not be the best men to form the nucleus of the Section of Physics and Chemistry?" There is here outlined an "Academy" which five years later became



the National Academy of Sciences. Hare was then no more. Henry became one of its earliest presidents.

Robert Hare died most unexpectedly on Saturday, May 15, 1858. The event was a terrible shock to his numerous friends at home and abroad. Society, as well as Science, mourned, in his departure from this earthly scene, the loss of a brilliant ornament. Indeed, his death created no ordinary sensation. The public press, throughout the land, bore splendid, eulogistic testimony to his solid worth. It spoke of him as simple, indeed child-like in his manners—easy of approach, singularly modest and retiring. He was said not to have had an enemy and “carried with him to the grave the warm and kindly remembrance of thousands who knew his rare qualities of head and heart.” Few men of his time were so universally respected by the scientific world. “He was a patient investigator and a man of capacious intellect,” was the testimony of Samuel D. Gross, the eminent surgeon. He was most cordially esteemed and honored for his incorruptible integrity. It required great force of evidence to unsettle his mind on any subject when once his conclusions were established. “Indeed, in a truly muscular grasp and unyielding tenacity with which he clung to his opinions, the firmness of his mind and the energy of his will were presented in their extreme aspects. But no dispassionate observer questioned the supreme love of Justice and the apostolic devotion to Truth which lifted him above the plane of the common mind, and rendered him invulnerable to the ordinary temptations of the world. He was as firm in his virtues as he was uncompromising in his opinions.” His life conduct was without spot and above suspicion. And it was also recorded that he was a high-minded and public-spirited gentleman—just and honorable in all his dealings—constant in his friendships—faithful in prosperity and adversity—ardent and disinterested in his attachment to his country—bold and zealous in the pursuit of truth.



ROBERT HARE  
In Advanced Age  
From a Photograph





“ There have been many more attractive lecturers—no one more earnestly intent in instructing his class; and, certainly, no one . . . performed his experiments on such a large scale, and with what might almost be called ‘ grand apparatus ’—more especially when he wished to exhibit the wonders of electricity. . . . It must have seemed to his auditors that when he sometimes paused in the very midst of an explanation, it was from a want of clear conception of his subject, or for words; not so—but because at the moment, a new thought would present itself, and he straightway allowed himself to imagine the new combinations and results that must follow.”

In person Robert Hare was portly, hale and prepossessing. He was above the middle height, had a dark keen eye and “ was vivacious and agreeable in conversation.” His head was large and of noble mould; no stranger could meet him without being impressed by a figure of such grandeur, and a head and features so remarkable.

He was an ardent patriot, “ who loved his country and cherished its institutions not for office or emolument, which he never sought or received, but, from pure and lofty motives. He was of the school of Washington—an enthusiastic admirer of that great man—a federalist, while that primeval party had a name and retained vitality, and when it passed . . . he was found among the Whigs. He occasionally wrote upon the great political and financial questions which agitated the public mind (pp. 21, 217). These discussions, like all his writings, were always marked by vigorous thought, large vision and elevated patriotism.” He loved literature and his philosophy was sometimes softened by listening to the Muses.

One admiring writer said: “ When I looked upon his majestic form a few months before his death, it was erect and commanding as ever before. He stood with manly firmness under the weight of many years, and walked with a measured



but elastic step, never bending beneath the burden. Not a nerve was unstrung, nor had his physical frame been materially enfeebled by the earnest labors of a long and useful life. In his organic structure and the Roman firmness of his character he was like the mountain oak, while the maturity of his mind was unaccompanied by the ordinary physical infirmities of old age. . . .”

And his old Faculty expressed its feelings of respect “ for the memory of one who has stood either in the relation of colleague or preceptor to each individual present ” in these words:

Resolved—

That the exalted character and brilliant career of Dr. Hare, whose far spread reputation both in foreign lands and in his native country, will long survive him, are a just source of gratification and pride to the department of the University with which he was so long connected, and which has been eminently benefited by his labors as an experimental chemist and philosopher.

That it is a subject of pleasing reflection, that his life, devoted to the calm pursuit of science, and an earnest desire to benefit his fellow beings, has peacefully terminated at a ripe old age, in the cheerful prospect of future happiness. And that these resolutions be entered on the minutes of the Faculty, and that a copy of them be transmitted to the family of the deceased.

R. E. Rogers,

Dean of the Medical Faculty.

When the Secretary of the Smithsonian Institution announced the death of ROBERT HARE, one of the principal benefactors of the Institution, and its first honorary member, Professor Bache gave an account of the life, character and scientific researches of Dr. Hare, and offered the following resolutions:

Resolved, That the Regents of the Smithsonian Institution have learned with deep regret the decease of one of the earliest and most venerated honorary members of the establishment, Robert Hare, M.D., of Philadelphia, late professor of chemistry in the University of Pennsylvania.

Resolved, That the activity and power of mind of Dr. Hare, shown through a long and successful career of physical research, the great fertility of invention, the happy adaptations to matters of practical life, and the successful grappling with questions of high theory in physical science, have placed him among the first in his country of the great contributors to knowledge, *clarum et venerabile nomen*.

Resolved, That while we deplore the loss of this great and good man, who has done so much to keep alive the flame of science in our country in past days, we especially mourn the generous patron of our Institution, the sympathizing friend of the youth of some of us, and the warm-hearted colleague of our manhood.

Resolved, That we offer to the bereaved family of Dr. Hare our sincere condolence in the loss which they have sustained by his death.

We have become acquainted, by the accounts given upon preceding pages in the letters there recorded and in the numerous communications published by Hare during the fifty years of his devotion to science, with his achievements. It is probably not so easy to evaluate them. His contemporaries, as seen, considered him a real leader and frankly acknowledged the high position to which he had attained. There existed no jealousy in their hearts. They recognized the value and high character of his contributions. Measured by present-day standards, many of these contributions lose their value, but even in retrospect Hare's labors cannot fail to fill the student of science with wonder. Let us transport ourselves in thought to the days in which Hare wrought. It



was—the beginning at least,—just as the Nineteenth Century opened, and what then was the condition of chemistry in our country? Who were the leaders and what had they accomplished? At that period Woodhouse was perhaps the ablest and most eminent representative of the Science. He was a pupil of Lavoisier and had studied in England. Hare always regarded Woodhouse as his teacher, as the one who pointed out to him the ways then known in experimental chemistry, but the very first product of Hare's independent thought—the oxy-hydrogen blow-pipe—far surpassed anything done by Woodhouse, or by any other chemical worker in this country. It was in truth an epoch-making contribution. Lavoisier may have burned oxygen and hydrogen together, but he absolutely failed to observe the intense heat of their flame. Hare discovered it and applied it. Chemists very probably were longing for greater heat to carry out some of their problems, or at least to learn what would occur if bodies could be exposed to temperatures higher than those to which they were accustomed. It was this desideratum which Hare brought to them. In more modern times chemists again sought high temperatures and when these were placed at their disposal through the electric arc by Moissan and Acheson astonishing results came forth. A new era opened up. So with Hare's discovery there came the ability to melt refractory bodies. In this class was platinum. The ease with which it was rendered molten and the readiness with which it could then be worked led to the inauguration of the platinum industry under the direction of a pupil of Hare—one named J. Bishop, founder of the well-known works located at Malvern, Penna. Another application of Hare's discovery was the lime-light—the Drummond Light. We are informed that these were used in light-houses on the coast to guide the mariner safely in his course. Our heart's gratitude went to Davy for the noble, humane

discovery made in the miner's lamp—the Davy Safety Lamp! No wonder thousands of noble toilers down in the bowels of Mother Earth gathered on one occasion to express their deepest gratitude for his efforts for them. Should the lighthouses be looked upon with any less regard? To those who go down to sea in ships—that bright beacon light on the strange shore does mean peace, comfort and safety; to its perfection Hare paid his tribute.

Places of interest to chemists are the great industrial plants using untold stores of electric energy to carry forward certain chemical processes. Among these may be classed the production of caustic by the electrolysis of salt, *using a cathode of mercury*. This last feature had its birth in that early experiment in the preparation of calcium, when Hare, for the first time, used mercury as cathode (p. 321) in the electrolysis of aqueous calcium chloride. It is a fact, and why should it not be acknowledged? He did not seek caustic, but the novelty was the employment of *mercury as a cathode in an aqueous electrolyte*. From the purely analytical side it is the fore-runner of all that in recent years has been made possible with mercury as a cathode. Considering these facts, this contribution from Hare surely has decided merit.

His attraction to electricity, his familiarity with the voltaic source of it, the knowledge of the accomplishments of Davy, his constant pondering on the fundamental problems elucidated by Faraday—all these, with the additional knowledge that none of the existing sources of voltaic electricity were satisfactory, carried him forward in his studies until he evolved the calorimotor, but better, in many respects, the remarkable deflagrator so highly prized by Faraday (p. 131). In these again Hare advanced the lines of human knowledge, laid foundations upon which others built to greater advantage. In it all there is manifest the pioneer, blazing the way for progress and improvement.



In the field of analysis, particularly that of gas analysis, Hare must be given a high place. He never developed a method for the determination of a metal or for its separation from its associates, or did he even improve some well-known procedure, but in studying his eudiometers, presented in such a variety of forms, the conviction flashes promptly over one's mind that herein he was a true pioneer. Much of his apparatus is cumbersome and at first glance difficult to manipulate, but on closer observation simplicity is noted and ease in manipulation makes itself felt as certain. The writer has no desire to detract in the slightest from the splendid forms of gas apparatus evolved during the last thirty or forty years, but a careful study of the forms described by Hare makes him feel that in them are many of the most cherished ideas of the later and more satisfactory apparatus. His hope is that the credit due Hare for his early efforts in this direction may be paid. Further, it is evident from Hare's writings that before very large classes he actually performed accurate gas analysis, and also demonstrated gas composition as is done to-day by many teachers with the elaborate and elegant apparatus of Hofmann.

Nothing, probably, produced such a profound impression for years as did the discovery of the electric furnace, and yet on turning to p. 315 we have described for us an actual electric furnace constructed by Hare, with which he succeeded in isolating calcium, in preparing calcium carbide, which gave to him acetylene (not recognized by him) and the most striking result of all the conversion of *Charcoal* (amorphous carbon) *into graphite*! How is all this to be regarded? Is it to be designated as primitive? Yes, it is that; but, does it not stamp its originator as a true pioneer in a field which to-day is cultivated most assiduously and extensively with astonishing outcome? Probably none of those who have developed the field of practical electro-chemistry have ever

read a description of Hare's furnace, but it was built by him and with its assistance he obtained products which then called forth little enthusiasm, then dropped from view, and which are now of common occurrence. A perusal of Hare's contributions, a thoughtful study of the various forms of apparatus which he devised cannot fail to impress one with the truth of the saying, "Despise not the day of small things!"

His excursions into the organic field did not lead to any remarkable conclusions; yet, they were pioneer efforts. From such meagre information as can be had, it is known that he never enjoyed any instruction on the problems in this domain, so that he pursued his customary method of trying out and recording the results which are most interesting and instructive. So it was in his isolation of certain elements, *e.g.*, boron and silicon, where the methods bear ear-marks or germs of later, more successful procedures. The gratifying feature about all of Hare's work is the fact that it distinguished its author as an actual doer. He wrote little, comparatively speaking, unless experiment obliged him to make record of his observations.

Upon Silliman the death of Hare produced a profound effect. His loss none could measure. As boys—yes, boys—for they had barely passed the teens when their paths crossed and together they began to sound the depths of chemistry by experiment. Through life they shared their problems. Indeed, the picture of their work is a most happy one to contemplate. Their correspondence, if it were possible to get it all together, but alas, this is beyond hope, would reveal a remarkable friendship. Reference has been made to that of Wöhler and Liebig, but with Hare and Silliman the ties were just as close, intimate and affectionate. It is doubtful whether Hare ever failed to acquaint Silliman with his difficulties, scientific or personal. The relations, too, of their families were so happy and intimate. The writer often dwelt



upon the possible feeling of Silliman when Hare, in the later years of his life, let his thoughts and interest be diverted to spiritualism. Naturally, he kept nothing from Silliman. A copy of the cherished volume on this subject was, of course, sent to that colleague whose opinions he sought and whose affection was to him as the breath of life. In casting about the following letter from Silliman was discovered. It has told the writer all he wished to know. It is a noble, lofty and grand letter; it was written quite early in 1857, and reached Hare before his death. It cannot but have made some impression upon the man whose life work we have been so intently reviewing. It reads:

“My dear Hare,

In return for your present at Albany, I request you to accept, as a proof of my kind regard and good-will, a small volume, entitled, “The Christ of History.” It goes to you by the mail which conveys this letter. As I have perused with respectful attention your work on Spiritualism, I ask that you will in turn read this little book, which presents a view of the Saviour, to my mind both original and convincing. Four historians, writing without consent and independently of each other, concur in presenting a character of celestial elevation and goodness,—such a character as has never been presented before or since in human history, nor conceived of by the mind of man. The narrative of his life, his acts, his teachings, his example, his death, and his resurrection, proves his divinity,—divinity associated with humanity, that thus he might be our brother in sympathy, both in joy and sorrow,—a union incomprehensible to our finite minds, but not more so than that of our immortal souls with our mortal bodies. The little volume which I now send you comprises, as you are aware, but a small portion of the copious evidence which supports the divine origin of the Scriptures. The Old Testament, marked by the peculiarities of the ages and coun-

tries which it commemorates, with occasional openings into the future world, holds out in prominent relief the interests of the present world; while the New Testament, in accordance with the prophecies in the Old Testament, brings life and immortality fully to light through the Saviour. Had your course of research been as fully devoted to these subjects as it has been to physical science, I trust you would not have been an unbeliever; and it is even now not too late to ascertain whether the Bible is really, as you intimate, a cunningly, or even a clumsily, devised fable. Should you, to say no more, view the "Christ of History" as I do, you may have occasion to review the position you have taken, which appears to me full of danger. I must confess that I closed your volume with very painful emotions, and my mind has anxiously balanced between the duty which it seemed to me I owed to my early and constant friend, and my despondency as to my power to produce any salutary effect upon his mind. At last, after much consideration, I have concluded to address to you a few remarks, in a spirit of perfect kindness and affectionate interest, but of deep and anxious concern. . . . My dear Hare, I cannot desert my Saviour,—him who spoke as never man spake, while he knew what was in man; who has paid my debt when I was bankrupt; and who sustained in my stead the penalties of a violated law;—I cannot desert him, and repose my confidence in the visions of so-called mediums. You and I are now old men, and the time is not remote—it may be very near—when we shall pass into the real world of spirits, into the presence of God, and, as millions believe with me, into the presence of the holy angels, and of the Saviour, and of the countless host of the spirits whom He has redeemed. You may remember that, at an early period, we conversed much and freely on the Christian faith; but, as we did not agree, and as I saw no hope of convincing you, while you, with a spirit of candor and kindness, appeared not



to wish to invalidate my belief, we tacitly dropped the subject. But, during more than half a century, we have maintained a friendly communion on matters of science, a warm personal friendship, with a frequent interchange of offices of kindness. I was unwilling quite to relinquish the hope that you would eventually become a believer in divine revelation, especially as a happy domestic influence on the part of one who, through many years, has worthily possessed your confidence, respect, and love, leaned altogether in the right direction. Your course as a man of science has been honorable, and duly and justly honored by your country and in other lands; while I, as your friend, have not been slow to proclaim your merits and vindicate your claims. It would have been happy if your public career had ended with science. . . . You will be hurt—I fear you will be offended—by my plainness; but when you realize that this is the strongest proof I have ever given you of that friendship which you yourself have valued, and which has been coextensive with our acquaintance, and almost with our lives, you will then perceive that these are indeed the faithful wounds of a friend. As one of your oldest and most faithful surviving friends, with a spirit grieved but not alienated, with hope depressed but not in despair, I have now relieved my mind from a painful sense of responsibility. I stand acquitted to my own conscience, to you and to God; and I earnestly pray now, as heretofore, that, under a divine influence, you may see the spiritual world, as I think I see it, through a divine revelation, commensurate with time and reaching through eternity. I will still hope that you may seek and find salvation through the Redeemer, and that through his intercession we may rejoice together in acceptance at the great day before the throne of God, our sins and follies being mercifully forgiven. Pardon me if, in my honest zeal for your welfare in both worlds, I have transcended the limits of that kindness and

courtesy which we have always maintained towards each other,  
and I beg you to accept this letter as a proof that I am still,  
as ever,                      Your faithful friend,

B. Silliman."

This may well bring us to the close of the life sketch of Robert Hare, an experimentalist of extraordinary ability. Therein lay the keynote of his great career. Those who have perused his controversial papers, especially those on the constitution of salts and halides of various kinds, find him a master even to-day. Such testimony is seen in the quotation on p. 215 from that master experimenter and investigator—Ira Remsen. But Hare's work speaks for itself, and we of the present surely rejoice in and are proud of his splendid contributions, of the fact that he was an enthusiastic pioneer in chemical science, that he won a place in the foremost ranks of the world's scientists and last, but not least, that he was "an American chemist."





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